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Ancestors or aberrants : studies in the history of American paleoanthropology, 1915-1940.

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ANCESTORS OR ABERRANTS
STUDIES IN THE HISTORY OF AMERICAN PALEOANTHROPOLOGY, 1915-1940

A Dissertation Presented

By

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ABSTRACT

Ancestors or Aberrants

Studies in the History of American

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February 1986

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The years between the two world wars, which just preceded the emergence of the neo-Darwinian "new synthesis," were intellectually difficult ones for paleoanthropology in America. Patterns of thought deeply ingrained in biology and anthropology pushed writers on hominid evolution into interpretive "blind alleys." Most prominent among the patterns was what Ernst Mayr has called "typological thinking," which often mixed with a tendency to project "scientific" racism back into the hominid past. A "splitting" habit in taxonomy combined with these and with belief in "orthogenetic" change to make polyphyletism the norm.

Hesitance to accept as human ancestors any Pleistocene forms exhibiting "primitive" characters led to phylogenies which put the known fossils on side-branches. Anatomically modern humans were thus left "ancestorless" by most writers, though nearly all continued to use

existing fossils in their evolutionary scenarios by designating them as "structural ancestors." Research conducted in Europe before 1914 on the Neanderthal skeleton and on the interperetation of endocranial casts, along with the Piltdown fraud, did much to establish these phylogenies and scenarios.

In tandem with these general themes came the ascendancy of several specific hypotheses that eventually clashed with accumulating evidence. That the brain had led the way in hominid evolution, that Neanderthals and other "low-brows" could be ruled out as ancestors, and that modern Homo sapiens had appeared early in the Pleistocene, became ever harder to maintain. The close evolutionary bond between humans and great apes theorized in England by Sir Arthur Keith and elaborated in America by William King Gregory remained vigorous, however, despite challenge.

The present study examines these issues through an analysis of the five Americans whose writings on hominid evolution were most extensive and varied -- Henry Fairfield Osborn, George Grant MacCurdy, Aleš Hrdlička, Earnest A. Hooton and William K. Gregory. The writings of each are analyzed separately, so that both general themes and responses to the changing state of the discipline can be traced. This approach reveals that shared patterns of thought did not prevent considerable diversity on nearly every main issue, a fact which rendered the field fertile for rapid growth later.

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The Nexus between "Speculation" and "Hard Data" in Paleoanthropology

Ever since Darwin's day fitting the human species into the pattern of evolution has been one of the critical tasks of evolutionary biology. The difficulties involved in doing so have made it a sort of gauntlet thrown down to evolutionists by creationists, and to Darwinians by evolutionists who believe that the mechanism of natural selection of small, random variations is insufficient to explain the beauty and complexity of life.[1] For these reasons the topic of human evolution has engaged the interest of a wide range of scientists and laymen, and has excited more than its share of controversy.

Much of the controversy has resulted from the fragmentary and equivocal nature of the evidence under discussion. Until the post World War II period the supply of well-dated, and relatively complete, skeletons of fossil hominids was small, especially of hominids older than the middle Pleistocene. Assemblages of stone tools, as well as of the fosslized remains of the animals that lived in proximity to the toolmakers, have been much more abundant, but the record they provide of the way of life of evolving hominids is difficult to interpret.[2]

The defects of the evidence only partly explain the fundamental divisions of opinion in paleoanthropology, and the twists and turns in the debate over human origins, however. As Niles Eldredge and Ian Tattersall have pointed out, the idea that further discoveries of fossils and tools will resolve all the major issues under dispute is a myth, a variant of the 19th century faith in gradual, inevitable progress that has been a potent force in shaping popular as well as scientific conceptions of the evolutionary process.[3] Differences in interpretation continue to exist because the interpreters bring different agendas and assumptions, both of a scientific and extrascientific nature, to the body of evidence available. Sometimes these sets of scientific priorities achieve the level of coherence, complexity and acceptance within a community of practitioners that allows them to be considered a "paradigm" as Thomas Kuhn has defined that term. For example, it can be argued that the style of prehistoric archeology practiced early in this century by people like George Grant MacCurdy, and especially what Ernst Mayr has called the "typological" approach to evolutionary biology that dominated paleontology and comparative anatomy prior to World War II, approached "paradigmatic" status.[4]

More often, though, the kinds of interpretive predispositions that paleoanthropologists have brought to their work are less developed and less explicit than this. Some fall into the category of what Robert Merton has called "theories of the middle range;" not highly abstract or systematic, they make sense of only a limited area of the field under study, involve only a modest set of assumptions, and generate a limited number of testable hypotheses. Others fit better into the category of scientific "themata" as described by Gerald Holton -- i.e. sets of assumptions and "tendencies" to characterize evidence in certain ways that are logically essential to the views expressed by a scientist but are themselves often implicit or understood. As Holton points out, such "themata" (for example the tendency to interpret natural events in terms of "life cycles," or the tendency to look for discreteness rather than continuity in natural phenomena) can cut across disciplinary boundaries; also, unlike "paradigms" they are not necessarily shared among all of the members of a research community. "Themata" are thus an important factor in the interrelations among various branches of science and in those between science and the broader culture.[5]

While the role of "themata" which are extrascientific in origin is probably of significance in all but the most abstract of disciplines, it seems clear that in paleoanthropology they wield a particularly strong influence. The "narrative" and "mythopoetic" aspect of evolutionary scenarios involving the emergence of humans has been frequently noted in recent discussions of the history of paleoanthropological theories.[6] That such scenarios might possess both a structure and function similar to the creation myths or heroic folktales of earlier eras should not occasion surprise. After all, the former point to qualities that are "natural" or fundamental for our species, that have served our ancestors in the critical struggles of the primordial past; they can thus serve to ground and confirm socially defined values and ideal character traits in the disturbingly contingent, "artificial" world that we moderns inhabit. These benefits seem similar in kind to those conferred by tales and myths.

The growing awareness among recent students of the philosophical and ideological content of various conceptions of human evolution has been associated with rising interest among paleoanthropologists generally in the history of their discipline. There seems to be in particular a desire to document the ways in which commitment to particular theoretical axioms or scenarios has inflenced the interpetation of the evidence and even the processes of research and exploration. While historical analyses such as the one that follows are very unlikely to generate new ideas for modern practitioners of paleoanthropology, they can be useful, for as Peter J. Bowler has noted, an awareness of past controversies can deepen one's insight into the fundamental problems that one's discipline cannot avoid dealing with.[7]

Because of the philosophical and ideological dividends that might be derived from drawing up a suitable scenario of human emergence, paleoanthropologists have often made them an important feature of their writings. This is not to say, however, that they have no purely scientific value as a description of the real world, though evolutionary scenarios in general have been called "untestable" and therefore "unscientific" by certain critics. Scenarios involve claims about the data in the fossil record that can be disproven, and often invoke general biological principles that are subject to laboratory test and logical analysis.[8] Therefore, past scenarios are not just so much "ancient history;" the way they related and to one another and to the data base on which they were erected forms one of the more interesting topics in the history of the discipline.

Scenarios are thus of a piece with the more rigorous aspects of paleoanthropology, such as the identification and dating of fossil hominid species, and the estimation of degrees of relationship among the various species identified; they need to be analyzed as part of the scientific record. Yet because of their character as narratives, they are also highly serviceable vehicles for the introduction of social attitudes and prejudices into the scientific content of the discipline. As Misia Landau has pointed out, though specific types of narrative such as the folktale have their own characteristic patterns of structural elements, a variety of actions can fill any given structural slot.[9] It is intuitively reasonable that scientists will employ analogies with their own experience or projections of their own codes of behavior, or both, in filling out their narrative reconstructions of the hominid past, and examples of the practice have been noted.[10] Once certain kinds of challenge, opportunity and response have been built into the picture of how humans "must have" evolved, a scenario can influence the way one interprets the "hard" evidence profoundly. Conversely, no matter what role a scientist might like his or her ancestors to have played in the world, some scenarios are not compatible with the "weight of the evidence" as it is defined by the surrounding scientific community.

Scope and Method of the Present Study

Originally, the present essay was projected largely to concern itself with scenarios, and in particular scenarios which purported to account for the evolution of human behavioral capacities. In the course of the research, however, the author discovered that until the 1950s, with the exception of writings addressed to a popular audience, there was very little paleoanthropological literature devoted to this topic produced in the United States. What there was turned out to be inseparable from the context provided by more detailed analyses of hominid morphology, primate anatomy and Paleolithic culture, in part, I believe, for the reasons suggested above.

Once it became clear that the focus of research would have to be widened, a reasonable time span had to laid out. The choice of the period from around 1910 to 1940 made sense for several reasons. First, this period embraces the publication dates of most of the major writings of the first generation of professional physical anthropologists in the United States, and nearly all of their writings devoted to paleoanthropology per se. Second, it includes the first group of synthetic accounts of human evolution written by American scientists which were considered comprehensive and authoritative in their day.

The years around 1910 also are important because they marked a major interpretive shift in paleoanthropolgy away from a monophyletic and toward a polyphyletic conception of hominid evolution, a conception in which "primitive"-looking fossils like the Neanderthals of western Europe were relegated to side branches of the human family tree. Though the shift has been discussed from a variety of angles in recent years, the way in which it developed and the arguments it generated in the United States have only been partially analyzed.[11] The process of conversion to the new orthodoxy in individual cases, the kinds of interpretive problems and inconsistencies it often evoked, and the increasing strains placed upon it by the discoveries of the 1920s and 1930s all present interesting issues for discussion.

The period around 1940 makes a useful terminus for the study because in the World War II era there began the major reorientation in the biological sciences that has generally been called the neo-Darwinian "new synthesis" by its proponents. The more or less rigorous use of "population thinking" that lay at the heart of the "new synthesis" was to have profound effects upon biological anthropology generally and paleoanthropology in particular. Though the transition was not an abrupt one, and perhaps slower to take hold than is generally believed,[12] it made sense to concentrate on the generation and a half of writers for whom this sea-change was not a major intellectual concern.

Another reason for choosing the 1910 to 1940 period is the fact that it marks the start of America's rise to significance in the study of paleoanthropology. For the first time a number of American scientists were clearly recognized by their European counterparts as major contributors to the debate over human origins. This is not to say that the contibutions of American thinkers were always original, or that there was an American "school" of interpretation. As the discussion of issues like the Neanderthal "displacement" theory, the question of whether humans had a "brachiating" anthropoid ancestor, and the interpretation of details on endocranial casts will make clear, many of the concepts and methods employed by American writers were derivative of European sources. Nevertheless, writings like those of William K. Gregory on the primate dentition, those of Aleš Hrdlička on the Neanderthal question, and in a more ephemeral sense, those of Henry Fairfield Osborn on the so-called "central Asian" theory of human origins were major events in the world of paleoanthropology in their day.

The basic methodolgy involved in the study is that of a history of ideas as they are revealed in the intellectual biographies of five main figures in American paleoanthropology active during the period. The individuals chosen -- Henry Fairfield Osborn, George Grant MacCurdy, Aleš Hrdlička, Earnest Albert Hooton, and William King Gregory -- were the most influential and most prolific authors writing on issues related to paleoanthropology. There were other Americans active in the discipline during the period, and several whose contributions were of note;[13] however, the writings of the latter did not have the breadth of coverage of the major issues or the extensive involvement over time in debates about human origins that the five named above displayed in their careers.

In dealing with the ideas of these individuals I have decided to trace the structure and development of each person's thinking on the subject separately. My hope in adopting this approach is to convey the sense of each mind responding to the intellectual problems and challenges of its era, and to portray the give and take of debate, as well as to analyze adjustments or failures to adjust to a changing body of evidence and changing trends in interpretation. Since the evidence and trends of greatest concern were often the same for all five scientists, this procedure is open to the charge of repetitiveness. However, the diversity of intellectual priorities and angles of vision in these individuals is such that a feeling of deja vu can generally be avoided; in addition, a more topical approach might have led to the loss of interesting material regarding these more individual themes.

A major factor underlying this diversity is that of the five figures involved in the study, only two, Hrdlička and Hooton, were full-time professionals in the field of physical anthropology; though MacCurdy had extensive training in biology, he was primarily a prehistoric archeologist. Osborn, while he did important work in comparative anatomy early in his career, won great reknown as a student of mammalian paleontology, and particularly of the evolution of large herbivores. Gregory was active both as a paleontologist and comparative anatomist, and did a great deal of work on the primates, especially the primate dentition.

That physical anthropologists were in the minority among the major students of paleoanthropology underscores the embryonic state that the former field was in during the early part of the twentieth century. In the period between 1900 and 1920 anthropology in general was only beginning to make its way into the academic curriculum;[14] physical anthropology was often taught and practiced as an adjunct to anatomy in medical schools or "ethnology" (i.e. cultural anthropology) in schools of arts and sciences. Though Franz Boas, the patriarch of early 20th century cultural anthropologists, had himself made some highly significant contributions to physical anthropology, he displayed a strong (and in some ways well-founded) suspicion of evolutionary explanations of the physical and cultural characteristics of modern "primitive" groups. This hostility had a great influence on the development of a "stand-offish" relationship between cultural and physical anthropology in America, and one that played a major role in inhibiting the growth of the latter discipline.[15]

Because the principal concern of this study is with scientific debate over the problem of human evolution, analysis is almost totally restricted to the published record. For this reason, the discussion will center largely on theories and "themata" internal to physical anthropology, prehistoric archeology, and evolutionary biology; conversely, the analysis of "external" or extrascientific factors in the development of each individual's views must necessarily be incomplete, especially analysis of those factors that relate to social attitudes more than to philosophical assumptions. Nevertheless, a significant amount of information about the latter concerns is available in the published work of the writers under discussion, and in the case of Osborn especially there is also data that reveals a great deal about the interrelation between his social and scientific views. Because, as has been noted above, "extrascientific" content is often a highly meaningful element in scenarios of human evolution, I have made note of themes that appear to fit this description, and have tried to account for them on the basis of the information available in the published record. I realize that these conjectures are tentative, but my hope is that they will provide a useful backdrop for further investigation.

A Preliminary Look at Some Major Themes

Though a detailed discussion would be superfluous here, some indication of the principal themes that will be raised in the chapters to follow is in order. First, it is significant that all of the scientists we will be looking at conducted much of their research and thinking, whether it concerned comparative anatomy, human paleontology, or skeletal biology of modern humans, in the older style of descriptive morphology that had changed very little since the 19th century. Thus, each writer was usually comfortable using individual specimens to represent morphological "types;" degrees of relationship between these types would generally be estimated on the basis of qualitative comparisons of allegedly critical characters, and hypotheses about phylogeny erected on the basis of these judgements. The problem of variability and how that might affect the definition of a "type" was at times a matter of concern, especially for the physical anthropologists Hooton and Hrdlička; but this problem was often neglected even by these individuals, particularly in general discussions of fossil hominid relationships. Also, concern with variability never led to questioning the very procedure of defining "types," especially "racial types," among past and present human populations.[16] More than any other characteristic, this concern with "typological" analysis lends the subjects of the present study a uniformity of outlook that contrasts sharply with the perspective of more recent thinkers on human evolution.

Another methodological practice now largely outmoded that was utilized by all five writers was closely related to the one above -- i.e. the ease with which specific and generic biological status was granted to individual fossils. Once one defined distinct "types" it was a short step to give those types the stable, hereditary basis entailed by membership in a "real" species or genus of their own. Once these designations were granted they tended to acquire a momentum of their own as well -- "types" became reified and were not often reevaluated.

A tool of morphological analysis that was also universally employed at the time and helped a great deal in the definition of "types" was the interpretation of surface pattern on endocranial casts from the skull vaults of fossilized or recent crania. Though the arguments underlying this practice had been explored earlier, its vogue in the English speaking world can be largely attributed to the influence of Sir Grafton Elliot Smith (1871-1937), an Australian neuroanatomist active in England who had risen to world eminence in the study of the primate brain.[17] By using the endocranial cast to reconstruct the convolutional pattern of the living brain, a diagnosis could be made of the mental capacities of the creature involved that could help to distinguish it from hominids of equivalent cranial capacity. This indirect method of taking a fossil's IQ proved to be very useful in dealing with "troublesome" fossils like the Neanderthals and Piltdown man.

The reference to Piltdown man brings up another set of common themes -- i.e. those that relate to substantive issues like trends in interpretation as well as difficulties of interpretation engendered by misleading pieces of evidence. One of the most persistent "themata" in early 20th century paleoanthropology, and one intimately related to "typological" thinking was the use of the "race" concept as an explanatory tool in assessing hominid relationships. The notions that discrete, hereditary racial "types" existed among modern humans, and that some races were more "advanced" while others were nature's stepchildren, were commonly accepted, and projected backward into the Pleistocene as well. This allowed scientists to draw analogies with supposed instances of modern "race conflict," with the inevitable result that the weaker race would succumb and be displaced by the stronger. Such analogies played a critical role in the interpretation of the Neanderthals, but they were also important in the evaluation of other "primitive" looking hominids. Though these analogies were displayed much less prominently in the work of Hrdlička than of Osborn, and the style of racial analysis employed by Hooton was much less "racist" than the latter's, the usefulness of the race concept and the belief in some sort of biological <u>"scala naturae"</u> among modern humans was shared by all five writers.

It is difficult to explain the prevalence of these ideas, but it is also clear that the decades around the turn of the century witnessed a great deal of scientific theorizing and research meant to demonstrate the "fact" of human inequality.[18] The usefulness of evolutionary racism as a rationalization for European imperialism has often been noted, but in the United States this connection cannot so easily be made. One might speculate that in a general sense Americans who were disposed to rationalize the existence of social inequality had to resort to science, since traditional religious and philosophical defenses had been undermined by the egalitarian axioms of both classical and reformist liberalism. In the case of Americans of British, Protestant heritage ethnic and class identification were often closely intertwined. "Race" provided an acceptable way of translating differences relating to ethnic and family background into biological "facts." For a person like Osborn the pattern of social relations of his native environment, metropolitan New York in the era of the "new immigration," as well as his own position within that pattern would clearly have seemed more "natural" according to such a theory. One might speculate similarly about MacCurdy and Gregory, though not with the same degree of confidence.

Another interpretive theme, more strictly "internal" to evolutionary biology than racial analogies, was closely related to the "splitting" habit in morphological and taxonomic analysis. This was the widespread belief that the norm for evolutionary trees was a pattern of long, nearly parallel branches reaching back into the remote geological past. This "polyphyletism" was sometimes accompanied by a belief in "orthogenesis," the idea that a each phylum was largely confined to developing further a set of specializations that appeared early in its history. Because of the voluminous and vigorous compilation of evidence from mammaliam evolution that he attempted to provide for these concepts, Osborn had a great deal to do with the spread of both "polyphyletism" and "orthogenesis." However, when he decided to pursue the study of human evolution in depth, the former idea had already been made an important part of the subject by the writings of Sir Arthur Keith (1866-1955) and Marcellin Boule (1861-1942).[19] Though no one else in the United States carried polyphyletism to Osborn's extremes, only Aleš Hrdlička failed to be strongly affected by it; this was especially true in regard to thinking about the later stages of hominid evolution. That polyphyletism could also be nicely integrated with a strong belief in the hereditary stability of racial lines is revealed very nicely in the writings of Hooton.

A theory that was more narrowly confined to paleoanthropology but made the application of polyphyletism seem reasonable was the brain-centered view of human evolution promoted by Elliot Smith. The belief that the human brain constitutes the supreme problem for an acceptable evolutionary theory of human origins goes back to the time of Darwin; in fact, the issue led to the break between Darwin and the co-discoverer of the principle of natural selection, Alfred Russell Wallace.[20] Elliot Smith's theory, which relied on preadaptations connected with arboreal life to "build up" early hominid brain power to the point where successful exploitation of a terrestrial habitat became possible, provided a naturalistic solution to the problem. It made humans an extreme case of an "encephalization" process that could be seen throughout the primate order; it was also flexible enough that it did not have to be wedded to any particular conception of the phylogenetic relationships among the major primate families.

For all these reasons the arboreal theory was highly regarded in the period under analysis. Indeed, one might say that it succeeded so well in establishing the plausibility of a brain-directed course of hominid evolution that some people forgot where the latter idea came from. How else can one explain Hooton's strong objections to the arboreal theory on the basis that it gave insufficient credit to the powers of "initiative" residing in the early hominid's superior brain?[21]

Not only did the "brain first" theory have wide currency, it also had an influence on other patterns of thought in paleoanthropology. The notion that the brain had begun to develop its unique characters quite early in hominid phylogeny, when combined with the great respect that scientists had for its present state of complexity quite logically led to the expectation that a great deal of time had been necessary to perfect this organ. As people naturally connected a nearly perfect brain with a reasonably modern-looking skull, "brutish-looking" Pleistocene hominids like Java man and later Peking man, along with the Neanderthals of course, became even more difficult to accept as human ancestors. This was doubly applicable to the australopithecines, the first representative of which was announced to the world in 1925.[22]

Paleoanthropologists began to expect forms of humanity closely approaching Homo sapiens in skull form and brain power would be found well back into the Pleistocene; the polyphyletic conception of evolution with the long, parallel lines of descent that it entailed squared well with this prediction and appeared to justify it. The terms "paleanthropic" and "neanthropic" came into general use to describe the two principal phyla involved, leading to the Neanderthals and to anatomically modern humans respectively.[23] The "race conflict" model then allowed scientists to dispose of the "paleanthropic" line in a struggle to the death (though not always a violent one) with Upper Paleolithic representatives of Homo sapiens .

The thorniest problem that American and European paleoanthropologists had to confront between about 1915 and 1940 was the following: where were the earlier representatives of the "neanthropic" line, the lower and middle Pleistocene Homo sapiens -like fossils that this style of reasoning seemed to demand? Various solutions were attempted, and a range of candidates proposed, but with the dubious exception of Piltdown man (and in the mid-1930s the incomplete Swanscombe skull, also from England) no fossil came forward to fill the role that had been cast; as more and more finds were made, in fact, the number of "low brows" multiplied. Indeed Piltdown man himself was generally not accepted in America as a direct human ancestor, for reasons that will become clear in what follows.

It is also important to note that the discovery of Piltdown man did not cause these theories, which Aleš Hrdlička believed (with justice) to have sent the study of human evolution up a "blind alley," to arise in the first place.[24] The fraud can be seen as having been "made to order" for accomodation to theories that were already gaining currency in England. This is not to say, however, that Piltdown was not one of the critical elements in adherence to the parallel phyla and early Homo sapiens theories. It gave great hope to those who believed that a "true" human ancestor would be unearthed eventually, and constituted the principal barrier in the path of those who like Hrdlička and Gregory were inclined toward accepting the "paleanthropic types" as representatives of necessary stages in the evolution of more modern forms. For these reasons its importance cannot be underestimated, and the way it was interpreted by scientists must be followed carefully. The "Piltdown problem" as it was viewed in the United States will consume a considerable portion of our analysis.

It is difficult to explain why paleoanthropologists in America and elsewhere were so willing to move in a direction that caused so many empirical and theoretical headaches. It does seem, however, that several of the ideas above are related to a more general "thema" that one encounters again and again in the period -- i.e. a strong tendency to isolate humankind from "brutish" relatives like the gorilla and chimpanzee in one way or another. In a famous essay on the australopithecines Gregory once referred to this syndrome as "pithecophobia." Its most extreme form -- the rejection of a close relationship between humans and great apes in favor of a much more ancient divergence of the hominid line from the primate stock -- was only endorsed by Osborn among the five writers discussed here. Still, a less extreme version of "pithecophobia" affected other writers at the time, and grew to be widespread among primatologists in the 1940s.[25]

Another "isolating mechanism" built upon a variant of the "arboreal theory" was employed by both Hooton and MacCurdy. It accepted the ape-human relationship but endowed the early hominids with a sort of free will, making them uniquely responsible for choosing their own evolutionary path to bipedalism, tool use and culture. Another way of treating human emergence as something of a "special case" was allowing for inner-directed, Neo-Lamarckian processes in human evolution even as one rejected their existence in lower animals; this was an attitude that Hrdlicka seemed to find congenial. The rejection of what Hooton once called the "gorilloid" types of fossil hominid as human ancestors can be seen as part of this pattern.

A final common theme in the writings of all of the scientists concerned in the study grows directly out of the lack of acceptable human ancestors noted earlier. This was the use of "primitive" forms that one had already declared to be off the main hominid stem as "structural ancestors." That is, by suitably redating the first appearance of the "types" represented by such fossils far enough back in time, one could consider them equivalent to structural stages that the main line must have passed through. Thus, one could illustrate the course of human evolution fully without admitting some or all of the major fossils to the charmed circle of human ancestry. This tendency has, I think, sometimes been included in the category of "morphological dating."[26] But the literal meaning of that term, to assign a date to a particular fossil based on its physical characters, actually describes a practice that was occasionally employed at the time, and that needs to be distinguished from this fixing of "typical" dates for physical "types."

Explaining why scientists so often resorted to "structural ancestors" is a difficult matter. Perhaps, to follow through on the folkloristic theme mentioned earlier, this practice represented the anthropological equivalent of attributing unpleasant types of parental behavior to stepmothers and stepfathers. All of the writers concerned conceived the story they were telling as that of the "ascent of man." Though it was not universal, there seemed to be a common aversion to seeing the "brutish" end of that ascending ladder directly below. Ironically, in these years between two catastrophic world wars, students of human evolution, who were so often accused of trying to reduce humanity to something lower, may actually have been trying to hold on to vestiges of the Enlightenment doctrine of the dignity of humankind.

C H A P T E R I

HENRY FAIRFIELD OSBORN, 1857 - 1935

Osborn's Career and Leading Principles in Brief

To put forward Henry Fairfield Osborn as one of America's major students of human evolution in the 20th century requires some defense. Many of his leading ideas were derivative and conventional. His speculations, when unorthodox, died a quick death at the hands of more careful theorists. He made no great field discoveries in paleoanthropology, and was never entrusted with the initial desription of one. Indeed, his greatest contributions to paleontology dealt with mammalian forms like elephants and titanotheres that were only distantly related to primates. Osborn did sponsor one highly publicized search for human ancestors, but the expedition failed to turn up anything that could reasonably have confirmed the high hopes that he had expressed at its outset.

Yet despite all these difficulties, Osborn's ideas remain worthy of close critical analysis. First of all, the conventional theories that he repeated and collected into an influential survey of human evolution provide insight into the intellectual climate of early twentieth century paleoanthropology. His more venturesome pronouncements carry some intrinsic interest of their own, not only because of the way in which they mirror his own social attitudes, but also because they forced less speculative writers to spell out orthodox theories more convincingly and stimulated major discoveries by others. Finally, Osborn's lack of a scientific following for his later ideas on hominid evolution should not blind us to his great influence on the general public, for through his writings and his institutional position he probably did more than any other American of his day to shape the popular conception of prehistoric man.

Osborn had a long, distinguished and influential career in the study of vertebrate paleontology generally, but the focus of the present essay will be his writings on fossil hominids and the principles underlying human evolution. The latter fall naturally into three phases: Osborn's initial forays into the subject of human evolution in the period around 1910; the major re-evaluation of Pleistocene hominid evolution that he undertook in connection with his 1915 book, Men of The Old Stone Age ; and most significant, his attempt during the 1920s to reinterpret the hominid fossil and archeological record and the data of comparative anatomy in support of his "central Asian" and "Pro-Dawn man" theories of hominid evolution. Osborn did not discuss the question of human evolution extensively until the second decade of the 20th century after his overall perspective on mammalian paleontology and evolutionary theory was quite fully developed. In fact, this perspective provides the context without which Osborn's pronouncements on the former issue cannot be thoroughly understood. This is especially true for the period after 1920. Thus, though the principal focus in what follows will be upon what Osborn said about hominids, his more general biological and even social views will be highlighted when relevant.

Henry Fairfield Osborn was born in 1857, the son of William Henry Osborn and Virginia Reed Sturges Osborn. Osborn's mother came from a family with long roots in Connecticut but his maternal grandfather, Jonathan Sturges, had become a succesful merchant in New York City by the time Osborn's parents met. William Henry Osborn was from a prosperous Massachusetts family; at the age of thirteen he had gone into a Boston firm specializing in the East India trade and had prospered, establishing his own firm while still a young man. At the time Henry Fairfield was born the elder Osborn was principally involved in the railroad business, however. The specific line concerned was the Illinois Central, and William Henry Osborn played an important role in its development by (to borrow William K. Gregory's pun) "engineering it through the panic of 1857" and guiding its expansion in the years that followed. As a consequence of Osborn senior's eminence in the business world and his mother's social prominence Henry Fairfield Osborn spent his youth, and most of his adulthood, firmly esconced within the elite "first families" of New York City.[1]

Though the Osborn family lived for the most part in the city during Henry Fairfield's youth, they built up a great attachment to the Hudson River Highlands region where they spent summers. Eventually the elder Osborn built a mansion, "Castle Rock," on a majestic, mountaintop location in Garrison, New York with a commanding view of the river valley; there Osborn himself was to do a great deal of his writing and to entertain distinguised personages during his years at Columbia and the American Museum of Natural History. While it is obvious that Osborn's parents provided him with great material advantages, those who knew him also noted influences upon his personal character as well. Osborn's brother William, for example, believed that their father's example as a "persistent, hard-working, hard-driving man of affairs" had a very strong formative impact; Gregory recounted the fact that Osborn himself was grateful in later years for his father's attempts to inculcate "habits of industry and self reliance" in his sons. Osborn's mother Gregory described as "a woman of genuine piety and humanity, who took a prominent role in works of charity and devotion in New York." Her strong Presbyterian faith seems to have had a subtle, but powerful effect upon her son's intellectual style; while he avoided vitalistic and supernatural explanations of biological phenomena, his emphasis upon the creativity, purposiveness and progressive nature of the life process seemed to reflect a desire to maintain compatibility between evolutionary science and a providential view of history.[2]

Osborn's formal education began with attendance at two small private academies in New York City, the Columbia Grammar School and the Collegiate Institute. In 1873 he entered Princeton University, whose President and leading spirit at that time was James McCosh (1811-1894). McCosh, a Presbyterian theologian and philosopher descended from the Scottish "common sense" school, was one of the very first prominent religious thinkers in America to make peace with Darwinism. In his view a "progressive" account of evolution, and one in which natural selection was supplemented in some way when the origin of higher order phenomena was involved, did not conflict with the essence of the Scriptures.[3] It seems reasonable to suggest, as Gregory did, that McCosh's perspective on the relationship between evolution and religion played an instrumental role in the development of what the former calls the "idealist" strain in Osborn's evolutionary ideas.[4]

It was not until Osborn was a college junior that he first began to develop a commitment to science as a career; he became deeply involved in paleontology through his studies with Arnold Guyot (1807-1884), a Swiss geologist then on the Princeton faculty. He participated in his first important excavation in 1877, when as just graduated seniors he and some classmates organized their own expedition to Colorado and Wyoming in order to collect fossils for Princeton's scientific museum. When he returned from that highly successful venture Osborn undertook graduate study in geology at Princeton for a time, but soon decided that the United States could not offer the thorough preparation in biology that he needed. In 1879 and 1880 Osborn studied embryology at Cambridge University under Francis M. Balfour (1851-1882), and comparative anatomy under T.H. Huxley at the Royal College of Science in London.

Returning to Princeton in 1880 on a fellowship, Osborn was appointed assistant professor of natural science in 1881, and professor of comparative anatomy in 1883. Osborn was highly successful in the next decade at Princeton both as a teacher and a researcher; he did important work in comparative neurology between 1883 and 1887, and thereafter on the paleontology of the early mammals of the so-called Age of Dinosaurs. The latter efforts laid the foundation for the "Cope-Osborn" theory of molar tooth evolution in mammals that was to be so important for William K. Gregory's work on the primate dentition.[5]

The stage upon which Osborn would play his role in science became a great deal larger after 1891, the year in which he was offered two newly created positions in New York -- head of the department of biology at Columbia and curator of the department of mammalian (later vertebrate) paleontology at the American Museum of Natural History. Both institutions were at the beginning of major periods of expansion, and Osborn had a great impact on the course followed by both through his researches, his choice of personnel, the expeditions he organized, and the young scientists he trained. Early in Osborn's tenure, in large part because of the close relationship he had built up with the pioneering American paleontologist, Edward Drinker Cope (1840-1897), the American Museum was able to acquire the latter's huge collection of vertebrate fossils. Building upon this base, Osborn directed a series of wide ranging geological and paleontological expeditions that eventually gave the American Museum the largest collection of vertebrate fossils in the world.[6]

It is a testimony to Osborn's energy, self discipline and ability to delegate tasks that he was able to continue his paleontological research at all considering the administrative workload he was shouldering. In fact, in the years between 1891 and 1910 he did much more than merely continue; he added a great deal of intensive work on the origin of mammals, and on the evolution and phylogenetic relationships of specific mammalian orders like the rhinoceroses and titanotheres, all of which culminated in an influential synthesis, The Age of Mammals . He also found time to write on general principles of evolution, and even on the history of the latter concept.[7]

Though Osborn's work as teacher, curator, college administrator and paleontologist gave him eminence in the scientific world, the post that gave him the greatest public recognition and influence was the presidency of the American Museum, which he took over upon the death of Morris K. Jessup in 1908. In his quarter century at the museum's helm, he played the role of "captain of science" on a grand scale,[8] supervising the construction of $11 million worth of new buildings and assembling $20 million in new collections and exhibits. He helped make the museum a world class institution, and as the New York Times obituary put it, lasting tributes to his efforts were sure to come from the many scientists using the museum's collections and from the "thousands of school children who gaze upon the Tyrannicus rex [sic]."[9] The exhibit mounted during Osborn's tenure that is most relevant to present study -- the "Hall of the Age of Man" -- must also have provoked its share of childish wonder; it was highly significant in addition as the repository of the two greatest sets of icons of the study of early man in America -- J.H. McGregor's three dimensional reconstructions of fossil men and the murals of scenes in the life of prehistoric peoples painted by Charles Knight. Nearly all the writings on prehistory published in the U.S. before World War II carried reproductions of one or both of these sets of images. The fact that Knight's work was done under Osborn's personal supervision added something indirect but undoubtedly important to the latter's popular influence.[10]

When William King Gregory used the phrase "captain of science" to describe his teacher and colleague, he did so with full knowledge of the phrase's implications.

Indeed, Osborn's background and his attitudes fitted him to manage his scientific pursuits in a style analogous to that of a business leader of the turn of the century. A brief analysis of that attitude and style will help illuminate several of the central ideas about humanity and its evolution that will come under discussion later on.

Since his birth and upbringing secured him membership in the White Anglo-Saxon Protestant business and professional elite, Osborn never developed that ambivalence toward the 'establishment' common in so many American academics who both crave the elite's acceptance and yet question its legitimacy. Thus in Osborn's career there is no evidence that he saw the professional pursuit of knowledge as a challenge to reigning social values. As they developed, Osborn's ideas about human evolution strongly confirmed the traditional American social order as he understood it; elitism and the habit of making racial distinctions were inseparable from his science and his politics. Indeed his scientific autobiography reads much like the memoir of a successful tycoon -- combining an elaborate resume with words of encouragement to the young man who would try to follow a similar path to success.[11]

It was probably this concept of the purpose of an autobiography as much as personal vanity that accounts for an odd feature of the book -- i.e., that honors granted and medals awarded to Osborn by various universities and scientific societies receive equal billing with purely intellectual events like theories he advanced, discoveries he made, and priorities he claimed. For Osborn the recitation of honors attained had an instructive purpose -- it showed the aspiring researcher that recognition by one's peers was a worthwhile, and an achievable goal, even if monetary rewards were lacking.[12] The currency of the scientific community, these honors measured success in a tangible and socially accepted way, and showed science to be the equal of other professions that gentlemen might pursue. Osborn's breeding was too good of course, to remind these other gentlemen that pecuniary calculations had not had to figure in his own choice of a profession.

That Osborn saw himself as a sort of scientific "captain of industry" also came out in his bearing toward colleagues. Gregory describes him as a man very conscious of his own dignity and importance, yet generous and indulgent toward those whom he had chosen to work under him. But this easy relationship was predicated on maintaining a clear hierarchy -- true collaboration with juniors was apparently very difficult for him.[13] Perhaps this self-conscious concern with leadership also helps explain the enjoyment he took in championing an unconventional theory, and the good nature he displayed in accepting the criticisms his subordinates made against these positions. He enjoyed being "out front", throwing his prestige behind a new idea, or at times an old one newly clothed in the neologisms of which he was so fond.[14] Osborn also wrote a great deal about the innovators and leaders of the previous generation, men whom he had known personally. The tone of these writings was not that of historical criticism, but rather of personal anecdote and celebration of past achievements, and they seem to reflect in part a desire to show that their author was carrying on a great tradition.[15]

Another professional duty that leading men of science, in Osborn's view, had to undertake was that of publicizing their findings. He called upon the example of Huxley to confirm the responsibility of "the man of science to devote a certain part of his time, however absorbed in research he may be, to an honest attempt to scatter scientific truth."[16] He discharged this duty to the public often in his career, but aside from the writings on fossil hominids that will be discussed below, there were two issues that brought him into the public forum more energetically than others -- the defense of the theory of evolution and the cause of racial purity. Both these issues reached political climaxes of a sort in

America during the 1920s -- evolution with the Scopes trial, and racism with the successful campaign to restrict immigration. In both climactic controversies Osborn played an active and prominent role; during the Scopes trial, for example, he acted as a scientific advisor for the defense. His involvement with immigration restriction was even more significant. Along with the racist Madison Grant, Osborn was a co-founder of the Galton Society, which took a leading role in the post-World War I "eugenics" movement. In addition, Osborn helped organize, and presided over the Second International Congress of Eugenics in 1921, whose main focus was the racial theories underpinning the movement to curb immigration, and the congress was instrumental in lending these theories an aura of scientific credibility.[17]

Professional responsibility and perhaps a sense of noblesse obilge could of course do much to render Osborn a willing disputant in these controversies. By 1920 faith in evolution provided the cornerstone of the paleontological edifice. But on the question of race and immigration class attitudes contributed as well. Like his close friend Madison Grant, Osborn believed firmly in the virtues of the Anglo-Saxon branch of the Nordic "race" that had supposedly built America and was still the dominant ethnic group in American society. Like Grant and other self-appointed spokesmen for this group Osborn worried about "race suicide" -- the outbreeding of his own superior group by biological inferiors, and about the growing political influence of immigrants from backgrounds and races that were alien and perhaps hostile to "civilized" values.[18] That the "scientific" racism which he shared with Grant, and with many of his colleagues in biology and anthropology, could provide "proof" for the sentiments voiced at the dinner parties and gentlemen's clubs could not help but awaken his spirit of service to class and country in the crisis times of the early 1920s.

Questions of race psychology and eugenics seem so distant from those of mammalian paleontology that one might wonder how Osborn could claim any scientific competence in them. One might, though to a lesser degree, apply the same criticism to his writings on human evolution *per se.* But the fact that he was willing to make these excursions from his area of greatest expertise brings up another important theme in Osborn's concept of the scientist -- the notion that a successful scientific career must have both an *intensive* and an *extensive* aspect. Though it was important for a scientist to maintain a primary focus on one area of intensive research, he asserted that specialization was not enough.

Only by undertaking productive work in a variety of areas could one develop the broad knowledge of principles and ability to conceptualize that would make fresh and original work possible.[19] Though this attitude can be defended on purely epistemological grounds, it also bears an interesting analogy with an aspect of the business world -- i.e. the phenomenon of entrepreneurship, where engagement in several "lines" develops an understanding of "business principles", and success in one specialty is not confining, but instead gives one the license to "branch out".

Osborn's Earlier Views on Human Evolution

As noted above, Osborn "branched out" into several areas during the course of his career, but for the purposes of this study the area that counts is human evolution. He wrote little about humankind before 1910, and that little dealt more with living races than with human paleontology.[20] The publication of his great synthetic work, The Age of Mammals , in 1910 appears to mark a watershed, for after that date writings on fossil man, prehistory and the problem of human evolution generally increase in frequency, and become most numerous in the 1920s.

Humankind does not get center stage in the Age of Mammals, but rather enters the story, as a major character though, at the proper chronological point in the drama --in this case the Pleistocene epoch. Still, the book is important both for the insight it provides into Osborn's conception of the general "laws" underlying the evolutionary process, and for the contrast that this first detailed discussion of fossil hominids provides with his later views on the subject.

Osborn relished the process of naming and cataloguing "laws" and "principles" of evolution, and produced lists of them often in his career. Four "laws" and one "principle", though, can be regarded as central to his understanding of the evolution of mammals. The first -- the law of "adaptive radiation"[21] hardly requires explanation, and is a hallmark of evolutionary thought: adaptation, over time, produces diversity in the descendants of a single stock, that is, when there are diverse environmental "niches" to exploit.[22] The "polyphyletic law," Osborn's second, follows directly from the first and the idea of species -- adaptation to diverse environmetal opportunities will cause the splitting of the descendants of a mammalian line "dwelling in the same geographic region" into multiple "side branches or series which we call phyla."[23] Thus stated, the law is not controversial; the problems come in the frequency with which and the circumstances under which one invokes it. Because the biological record is notoriously imperfect and because the actual pattern of life and reproductive behavior of fossils is hard to reconstruct, the drawing of phyletic "trees" has often involved a strong element of aesthetic taste and personal prejudice. As will become clear later on, Osborn preferred richness and variety in his picture of the tree of life.

The third law -- that of "analogous evolution" raised similar problems of interpretation. Again Osborn was codifying a well-known phenomenon, in this case that of "convergence." The exploitation of similar environmental opportunities often causes species which are not closely related by descent to show similar adaptations in both function and structure.[24] Osborn rightly pointed out that if one wanted to avoid false inferences of recent common ancestry between fossil forms one had to be able to distinguish these analogies, whose cause was likeness of function due to convergence, from homologies, which were true likenesses of structure that revealed common descent. "Analogous evolution" however, involved the same problems of taste and judgement as polyphyletism -- with fundamentally different groups such as porpoises and sharks analogy and homology were relatively easy to distinguish, but when one was comparing apes and humans, or different varieties of fossil hominid, one man's analogy could be another's homology, and *vice versa.*

The fourth law -- which proclaimed the "irreversibility" of evolution -- was even more problematic since it could easily be misinterpreted as misapplied. Often called "Dollo's law" after Louis Dollo (1857-1931), the scientist who gave it its clearest statement, "irreversibility" refers to a relatively non-controversial observation, namely that the uniqueness of evolution as a historical process and the complexity of its genetic underpinnings make the appearance of a structure in exactly the same form at two points in time unlikely. Thus, once a structure is lost, or altered in a significant way, its original form cannot be regained.[25] What was controversial, however, was the question of how inclusive the term "major structure" should be. Osborn took a broad view, and even considered characters such as the relative proportions of limb bones subject to Dollo's law. In his later writings on the primates this would loom as a major point of contest with his critics.[26]

The last idea in *The Age of Mammals* that would prove highly significant for Osborn's later views on human evolution was not so much a law as a principle of biogeography -- the study of the distribution of plants and animals. In analyzing the past and present distribution of mammalian groups Osborn had come to the conclusion that the "Holarctic" region, a broad band around the earth that included the northern part of Eurasia and most of North America, was the principal center of mammalian evolution; most of the orders of mammals, he believed, had made their first appearance there rather than in the tropics or the southern hemisphere.[27] Though the idea as stated in 1910 was in itself controversial, it is important to note that later statements of the principle by Osborn and his junior colleague at the American Museum, W.D. Matthew, would be more extreme.[28] In 1910 Osborn was not defining the notion of a "center of evolution" rigidly; the "Holarctic," after all, took in a lot of territory, and he was willing to give Africa credit for several "autochthonous" orders of its own. In addition, by focusing on <u>orders</u> of mammals, which are broad groups whose first members appeared in the very distant past, he left open the chance that more recent events, like the emergence of modern genera and species, could have occurred in other places as well.[29] In the years to come all of these qualifications would receive less emphasis.

This image of relative caution becomes more pronounced when one looks at Osborn's treatment of fossil primates, and especially hominids, in the Age of Mammals. On these issues he shied away from strong opinions, deferred to European authorities on key points, and noted problems about which differences of opinion among the authorities still precluded sure judgement. Indeed there were several major issues on which he followed his authorities to conclusions very different from those he would express later. For example, in discussing the dryopithecines, a fossil anthropoid group then known largely from deposits of the Miocene epoch, Osborn reserved judgement on the key question of whether they represented a "stem form" from which both humans and great apes had evolved.[30] Later he would reject this idea vehemently.

On a closely related matter, the earliest appearance of recognizably human primate forms, Osborn also opted for a conservative point of view. Like other writers of the time, he felt that the presence in ancient geological deposits of "eoliths," pieces of irregularly fractured flint that might have been used as tools by early hominids, gave "pre-human types" potentially great antiquity.[31] But he made sure to emphasize the qualification "pre-human," for it seemed to him "very unlikely ... that any being at all closely resembling man (genus Homo) could have remained through such long ages while all other genera of mammals became transformed."[32] Indeed, he concluded, the "only known Miocene and Pliocene primate which might be considered an 'eolith' maker" was the primitive anthropoid genus Dryopithecus.[33]

Restraint and reliance on conservative authorities marked Osborn's treatment of Pleistocene hominid evolution as well. Matters of dating were still unsettled. Albrecht Penck's (1858-1945) system of four main glacial stages separated by milder interglacial stages of varying length was still too recent to have settled into orthodoxy. While he accepted it provisionally, Osborn also made note of rival systems such as that of Marcellin Boule (1861-1942), and pointed out a major reservation of his own -- that Penck's estimate of the length of the Pleistocene, between one-half million and one million years, would extend the beginning of the Tertiary era back to twenty million years B.P., and of "pre-Tertiary time into hundreds of millions."[34] This major extension of the evolutionary time scale obviously made him uncomfortable, but he would not reject it, either.

Osborn also followed Penck on the placement of prehistoric tool traditions within the Pleistocene. The earliest tradition then known, the so-called "Chellean" (now often called "Abbevillean")[35], was dated at the Second, or Mindel-Riss Interglacial. The importance of this dating was that, even though it put the Chellean earlier than did some competing schemes, it still kept the earliest recognizable appearance of human culture in the middle Pleistocene,[36] and thus provided another argument in favor of the late appearance of forms "closely resembling man." Any movement that Osborn might later make toward belief in human emergence before the Pleistocene would run counter to this critcal piece of evidence, for the idea that the Chellean did not begin until well after the onset of the Ice Age was an archeological "given" well into the 1930s. A convincing theory of pre-Pleistocene *Homo* would require a truly recognizable tool industry before the Chellean.

Another key issue on which Osborn repeated turn of the century orthodoxy in *The Age of Mammals* was the question of whether human evolution in the Pleistocene had been unilinear of multilinear; he came down squarely on the side of unilinearity, both in culture and morphology. He adopted the traditional scheme of the Paleolithic worked out by French archaeologists, who pictured each of the major tool traditions as a separate stage or "epoch" of culture, and believed that each epoch represented a discrete rung on the ladder of cultural evolution.[37] As with culture so with the existing forms of fossil man -- Osborn discussed them in order of presumed antiquity, and did not reject morphologically "primitive" finds like Java man and "Homo heidelbergensis" as potential ancestors of modern man.[38] Neanderthal man, whom nearly all would reject as a direct human ancestor by the 1920s, was treated as _the_ form of man extant during the "Mousterian epoch."[39] Though he readily accepted the existence of several "distinctly simian or pro-human" characters in the Neanderthals which Boule had newly identified, Osborn maintained the view that the Neanderthal "species" was a fitting representative of "mid-Pleistocene man."[40] As such, the Neanderthals retained, for the present, a secure place on the main branch of the human family tree in Osborn's phylogenetic scheme.

Men of the Old Stone Age

The research that Osborn had done into the literature on human evolution for _The Age of Mammals_ had apparently awakened a desire for first hand acquaintance with the evidence. In August 1912, he undertook a "motor tour" of the principal sites in what was then the center of discovery about prehistoric man -- southern France and northern Spain. The tour provided perfect material for a popular article; in the ensuing report, entitled "Men of the Old Stone Age," that he wrote for the American Museum Journal (the forerunner of Natural History), Osborn focused on what had been his most memorable encounter -- that with the great cave art of the Upper Paleolithic.[41] Since the article was mainly descriptive, it included little of theoretical import. He sounded one theme, though, that would re-echo though all of his later writings on fossil man -- following the Abbe Breuil, be attributed the glories of Upper Paleolithic art to one people, the so-called "Cro-Magnon race," and saw the art and culture of "this great hunting and artistic race" arranged in a progressive series of developmental stages.[42] Both the near worship of "Cro-Magnon" man and the rigid identification of cultures with distinct racial types could become central to the further development of his ideas on humankind.

The next, and most important, step in this development came in 1915 with the publication of Osborn's magnum opus on the problem of human evolution -- the book Men of the Old Stone Age. Its aim was to provide the educated layman with both description and analysis of all the major pieces of evidence -- from geology, paleontology, and archaeology -- bearing on prehistoric man. It appeared to meet a real need, for as the distinguished paleontologist J.C. Merriam (1869 - 1945) noted in his review, despite the wealth of both popular and scientific writings on the "origin and early history of the human family" being produced in Europe, "comparatively little" had come to the American reading public "through our own literature."[43] Being readable and authoritative as well as American, Men of the Old Stone Age quickly established itself as a standard work on its subject. This of course makes an examination of its major themes and conclusions doubly important.

One crucial theme that Osborn's book shared with nearly all contemporary treatments of fossil man was that of "typological thinking," the habit of defining races, species and even genera on the basis of individual fossils, and often fragmentary ones.[44] As biologists have pointed out again and again since the emergence of the "synthetic" theory of evolution in the 1940s, this practice flies in the face of a fundamental fact about organisms -- the importance of individual variation within species. In 1915, however, when scientists discussed the earliest hominid fossils that had so far come to light, the temptation to indulge in typological thinking had to be strong. After all, Java man (a.k.a. "Pithecanthropus erectus") was only known through a single skullcap, a questionable thighbone and two questionable teeth; the so-called "Homo heidelbergensis" through one mandible; and the fraudulent Piltdown man (a.k.a. "Eoanthropus dawsoni") through skull and jaw fragments of at most two individuals at the time Osborn was writing. That the habit involved more than mere convenience, though, becomes clearer when one looks at the way in which both Osborn and others used Marcellin Boule's description of one skeleton -- the so-called "old man" of La Capelle-aux-Saints -- to define the Neanderthal "type", despite the existence of several other skeletons of similar age and comparable states of preservation.[45]

Men of the Old Stone Age shares another theme, or rather an interrelated complex of themes, with other works of its era -- a complex that revolves around the problem of polyphyletic or multilinear evolution. Osborn, like others in the period 1910-1920,[46] was moving away from the previous unilinear conception of human evolution, and this was the work in which he revealed that break from the old orthodoxy. There were barriers, though, that stood in the way of a convincing multilinear theory that encompassed both the morphological and cultural facets of the evolutionary process. First there were two conflicting assumptions of paleoanthropological thought --

1) the basically unilinear scheme of cultural evolution adopted from European archeology, in which each main Paleolithic industry was taken to represent a descrete stage on a ladder of increasing cultural complexity and 2) the rigid identification of specific "races" of fossil humans as the necessary carriers of specific Paleolithic "cultures." The second barrier was the dearth of fossils that could demonstrate the simultaneous presence of multiple hominid stems; as more and more of the extant fossils were put out on side branches the purported main line leading to Homo sapiens became especially hard to locate.[47] Under these conditions the possibilities for matching recognized tool types with specific fossil hominids at a comparable stage of development on each supposed evolutionary line were distinctly limited.

Since the need to answer a simple question -- how did man become what he is? -- pressed upon every author writing about prehistory, and the popularizers above all, ways around these barriers had to be found. Osborn found two routes that would be travelled by other American authors as well. The first route involved using fossils from supposed "side branches" and industries associated with those fossils as stand-ins for hypothetical points along the main evolutionary line. The reasoning, either explicit or implicit, was as follows: even though fossil (or industry) A is too primitive or specialized to be a true human ancestor, it represents, in some of its characteristics at least, a stage A' that human ancestors must have passed through, either at an earlier time or at the same time elsewhere -- perhaps in an environment more favorable to further "progressive evolution." The second ploy solved the problem posed by evidence that threatened to contradict the assumption of polyphyly -- i.e. cases in which fossils or cultures supposedly from different lines occupied successive layers in the same geological deposit. The simplest explanation, that of *in situ* evolution, was of course unattractive to polyphyletic theorists but there was also a simple and satisfactory alternative -- the invasion of the site under analysis by an "advanced" racial group which then totally displaced the previous, inferior inhabitants.

The association of polyphyletic theories and racist analogies of a sort used often in turn-of-the-century political debates about imperialism and colonialism in Osborn's work was not accidental, of course, and it raises the "chicken or egg" problem. Did he apply racist analogies to early man because he found it an easy way to preserve the polyphyletic theories that he defended in his paleontology, or did he find polyphyly intellectually appealing because it confirmed his deeply felt prejudices about the world of man and society? Perhaps the dichotomy is a false one and thus unresolvable. All that really needs to be said is that notions of racial superiority, conflict and replacement and polyphyletic evolutionary theories formed a very comforting cultural mix for Osborn. Indeed, the more committed he became to his racial program, the greater became his insistence on the great antiquity of the various lines of hominid evolution. In any case, some illustration of Osborn's reasoning in important instances is necessary to see just how he supported and developed his polyphyletic scheme.

If there was one practice that nearly all multilinear theories of human evolution adopted, it was the tendency to put the morphologically most "primitive" or "ape-like" hominids off the main evolutionary line leading to modern humans. The fact that these forms were generally the oldest was not enough to overcome reluctance to conceive of them as our ancestors. Thus, Java man, which Osborn dated at the boundary between the Pliocene and the Pleistocene, seemed too primitive to have been a common ancestor both for Neanderthal man and for _Homo sapiens._ [48] "Homo heidelbergensis," a creature of the Second Interglacial epoch according to its faunal associations, now appeared to Osborn to be a "Neanderthal in the making,"[49] which, as we shall see, removed it from human ancestry as well.

The other allegedly "primitive" form, Piltdown man, created unique problems of interpretation because of its hybrid character, but Osborn was able to resolve them to the same effect. That its skull form and cranial capacity could fit within the range of modern human skulls could be counter balanced by the great thickness of the cranial bones and the form of its endocranial cast, which the comparative anatomist Grafton Elliot Smith had found to be the "most primitive and ape-like" hominid brain excepting that of "Pithecanthropus" so far recorded.[50] If, as Osborn believed, Piltdown man was a truly primitive form, its relationship to the other two early hominids became a puzzle, since the Piltdown skull did not possess the low vault and massive brow ridges of "Pithecanthropus." His solution was to put Piltdown on a hominid side branch all by itself.[51] One difficulty that he did not have to overcome in classifying "Eoanthropus" as a true hominid was the presence of the extremely ape-like jaw that had been found with the skull -- for the time being he like other American students of evolution accepted the conclusion of Gerrit S. Miller, a biologist working at the U.S. National Museum in Washington, that the jaw belonged to some species of extinct chimpanzee.[52]

Even as Osborn was removing primitive fossils from the main line of evolution, he was trying to use them to illustrate stages in the evolution of human mental and cultural capacity. Thus, Java man, whose low skull vault contained a brain of very primitive character -- with an estimated capacity of between 855 and 900 cubic centimeters and an allegedly poorly developed frontal region[53] -- was still seen as probably able to make and use "primitive implements of wood and stone."[54] This in turn made "Pithecanthropus" a fitting representative, in Osborn's view, of humanity at the close of the "eolithic" stage, the long period of cultural evolution before the emergence of clearly defined tool types, which was just the stage, he thought, that one would <u>expect</u> to find the human family in at around the "dawn of the Pleistocene."[55]

Since Osborn estimated Heidelberg and Piltdown man to be mid-Pleistocene forms, they both should have represented a slightly later cultural stage than Java man in order to preserve the neatness of his evolutionary scheme, and so he found. The stage chosen for each was the so-called "Pre-Chellean," which Osborn defined, after the archeologist Henri Obermaier, as the very early stage of human invention in which flint workers were not shaping their tools according to a conscious design, but were "dealing rather with the chance shapes of shattered blocks of flint, seeking with a few well-directed blows to produce a sharp point or a good cutting edge. This was the beginning of the art of 'retouch.'"[56] How this would differ from the later stage of the "eolithic" embodied by Java Man is difficult to see, but presumably the advance that the "Pre-Chellean" entailed hinged on the definition of "well-directed" and the relative goodness of a "good cutting edge."

Not only was there some vagueness in the definition of the "Pre-Chellean", there was also willfulness in the attribution of the two fossil forms to that stage. Osborn had dated Heidelberg man at the Second Interglacial epoch, as we have seen, but by 1915 he had swung against Penck and toward Breuil and Boule in placing the start of <u>both</u> the Chellean and Pre-Chellean cultural stages in the <u>Third</u> Interglacial.[57] The most that could be guaranteed for Heidelberg man was thus an "eolithic" level of cultural capacity, but, evidently to underscore the advance over Java man that Heidelberg supposedly represented, Oborn was willing to concede the latter "pre-Chellean" status. He could speculate freely on this matter in light of his own assumptions, since there were neither artifacts nor an endocranial cast to check his findings against.

In the case of Piltdown Osborn had to work within the confines provided by both an endocranial cast and artifacts, but fortunately the hoaxer had supplied ones that could be squared with Osborn's conception of where the fossil ought to have stood culturally. In fact, the presence of a very "primitive" implement of "Pre-Chellean" affinities had helped confirm Osborn in the belief that the deposit in which the Piltdown skull had been found was truly of Pleistocene date.[58] Of course, the wish had probably fathered the thought here as well as on the question of whether the skull and endocast were truly primitive; because Osborn wanted to believe the fossil to be ancient and its deposit undisturbed, he accepted interpretations of the endocast and implement that fit an early epoch in the human cultural ascent.

The problem of matching fossils and cultures also existed for the later Pleistocene epochs, but for the initial part of this time span, the terms of the problem were reversed. Instead of having fossils whose cultural associations were unknown, Osborn had to guess at the morphology of the unknown makers of Lower Paleolithic -- Chellean and Acheulian -- implements, since no fossils had yet been found at sites referrable to either "culture."[59] His equivocal handling of this issue contrasts also with the bold treatment of Piltdown and Java man noted above. At one point he described the Acheulian quite baldly as the "early industry of the

Neanderthal races;"[60] later he asserted merely that the makers of Acheulian tools were at least "partly of Pre-Neanderthaloid race."[61] Since they are nowhere explicit, one can only speculate about his reasons for not wanting to identify the Chellean and Acheulian fully with the Neanderthal "race." Two possibilities suggest themselves: first, that he wanted to leave open the possibility that <u>some</u> predecessors of <u>Homo sapiens</u> could have inhabited Europe during the Acheulian epoch, and second, that he respected the cultural advances made during the Chellean and Acheulian too much to attribute them wholly to the Neanderthals. The latter conjecture gets some support from Osborn's attempt to contrast the advances made during these "epochs" with the "marked retrogression" in toolmaking technique that had allegedly occurred in the Mousterian, the industry which of course was most closely identified with the Neanderthals.[62]

Even if one cannot be sure if it lay at the base of Osborn's equivocation about the Lower Paleolithic, an unwillingness to grant Neanderthal man his previous place of honor in the human family tree was an explicit, and central feature in <u>Men of the Old Stone Age.</u> In his discussion of the Middle and Upper Paleolithic he made the Neanderthals an extinct side branch without genetic representation in modern human populations and the principal example in support of his polyphyletic theory. In accordance with this viewpoint, instead of conceiving the succession of the Neanderthals in Europe by Upper Paleolithic populations as a case of continuous evolution, he portrayed it as a discontinuous process of racial displacement, a process leading to the extinction of the "inferior race."[63]

By the 1920s the "inferior race" explanation of the disappearance of Neanderthal man had become almost a reflex among popular writers in America, but it is important to note that in 1915 Osborn was presenting it as a relatively new viewpoint to which there were plausible alternatives. For example, he pointed out that attempts had been made by archeologists to trace a gradual transition between Mousterian and Upper Paleolithic industries, the type of transition that would have been caused by in situ evolution rather than invasion and displacement of one culture by another.[64] On the issue of morphology, he cited the opinion of Ales Hrdlicka, a noted American anthropologist, that some Neanderthal fossils varied in a more "modern" direction than "typical" members of the group, as defined by skeletons like that of La-Chapelle-aux-Saints.[65] And finally, the notion that Neanderthal man fell far short of his successors in mental ability was undermined by the large size of the Neanderthal brain, which fell at least within, and perhaps above, modern averages as measured by cranial capacity.[66]

Why then did Osborn reject these ideas and pieces of evidence supporting evolutionary continuity, and adopt a theory of invasion and displacement, even when it rendered the cultural and physical origins of anatomically modern humans totally mysterious? The movement of European students like Breuil and Boule in this direction obviously had some influence on a writer like Osborn, who was not doing original research in the field.[67] But we must look further at Osborn's assumptions and reasoning, for not always did the conventional theories of the European experts recommend themselves so strongly.

Osborn's general confidence in the explanatory value of the concept of race was apparently the key element in his conversion to the Neanderthal replacement theory. He firmly believed the characters which distinguished racial "types" from each other were both strictly hereditary and extremely stable over time. In discussing the differences between the Cro-Magnons and the Neanderthals,[68] he asserted that

> once established these racial types are stable and persistent; their head form, their bodily characters, and especially their psychic characters and tendencies are not readily modified or altered; nor are they in any marked degree blended by crossing. Crosses do not produce merely blends;

> they chiefly produce a mosaic of distinct characters derived from one race or the other.

Ironically, this concept of race was in itself a "blend" -- by combining the "scientific" racism of the late 19th century (the belief in distinct "types," psychic "tendencies," etc.) with the "bean bag" genetics of the early 20th ("crosses" and "mosaics") an amalgam was produced whose ideological power was formidable. Interestingly, the rigid hereditarian bent of this concept of race was a later development in Osborn's thought. In the early part of his career he had followed Edward Drinker Cope in assigning a great role to the neo-Lamarckian principle of use inheritance in his conception of progressive evolution.[69] Like many other American biologists in the period between 1890 and 1910, however, the failure of scientists to provide experimental confirmation of this principle and the rise of Mendelian genetics had caused Osborn to question Neo-Lamarckism. It was also characteristic of these biologists to derive from Mendelism, and from August Weismann's (1834-1914) theory of functional independence of the "germ plasm" (the substance that transmits heredity) from the cells of the body, a strong belief in the dominance of heredity over environment.[70] From Osborn's vantage point in 1915 it must have seemed that hereditary racial differences were a more important factor in explaining human evolution than even he himself had previously understood.

If one wanted to picture the differences between the Neanderthals and Cro-Magnons as analogous to contemporary racial differences, several conclusions about the Neanderthal question would easily follow. First, the great morphological differences that Boule *et al.* discerned between the "classic" or "typical" Neanderthals and the Cro-Magnon "type" must have been the result of a long period of separate phylogenetic development; given the stability of racial characters, a long time must have been necessary to have produced such a degree of divergence from the last common ancestor.[71] A second, and logically stronger, conclusion was the idea that even though the Cro-Magnons had followed the Neanderthals in time, evolutionary transformation of the latter into the former was highly unlikely, since races once formed changed slowly, if at all.[72] Finally one could also argue that the absence of any fossils indicating a "mosaic" of Neanderthal and Cro-Magnon racial characters would prove that the "inferior" group had been totally displaced by the "superior" -- since no racial crossing had occurred.[73]

As the physical anthropologist C.L. Brace has observed in his own writings on the Neanderthal problem, there is a curious non-evolutionary or even anti-evolutionary aspect to the invasion-replacement model outlined above. Genetically and morphologically fixed "races" appear on the evolutionary scene and do battle, but the process of organic change over time -- i.e. evolution -- occurs offstage. Brace, much to the ire of his critics, has attributed this aspect of the model to the survival, especially among French anthropologists, of "catastrophist" biological theories.[74] For Osborn, though, it seemed to be the seductive comparisons with contemporary racial problems that attracted him to the replacement model. It was not so much that races were not produced by evolution as that they were produced <u>separately.</u> When brought together, their "natural" role was not intermixture but brute competition. The "natural" result of such conflicts, unencumbered by "unscientific" ethical notions like that of human equality, was the establishment of dominance by the superior race.

If French "catastrophism" was not a major element in Osborn's case against Neanderthal man, another idea characteristic of French thinking on prehistory was employed -- the theory of racial "degeneration." According to this view, races no less than individuals passed through a life cycle; old age, marked by progressive weakening of both physical and mental powers, must inevitably follow the vigor of youth. The alleged crude simplicity of late Mousterian artifacts was taken to reveal the existence of the degenerative process, which Osborn felt had been hastened by the onset of the rigorous fourth, or Wurm, glaciation.[75] That degeneration was not the unique fate of the Neanderthals but a more general phenomenon came through in Osborn's discussion of a later cultural transition, between the Upper Paleolithic and the Mesolithic, which he took as possible evidence for a decline in the "artistic energies" of his beloved "Cro-Magnons."[76]

Ruling out the Neaderthals as possible ancestors of modern humans would be much more plausible if one could establish not only that they were a very different race from their Cro-Magnon successors, but a clearly _inferior_ one as well. What if, in addition to inferential evidence about "degeneracy," one could produce evidence from a recognized authority on Neanderthal man that the latter was more "simian" both in body and brain than had hitherto been believed? This of course was the essential message of Marcellin Boule's work on the La Chapelle-aux-Saints skeleton, and Osborn's _Men of the Old Stone Age_ reflects the influence of this work upon the American scientific community in its strongest form. It was Boule's monograph, filtered through several layers of popularization, that defined the stereotype of the Neanderthal for the first half of the 20th century -- head slung forward, low forehead with massive brow ridges, shuffling, bent-kneed walk, etc.[77] Far from subjecting Boule to critical analysis, Osborn acclaimed the former's monograph on La Chapelle as an "almost faultless" work which had "aroused world-wide interest in the Neanderthal race."[78]

In addition to fixing the standard image of the Neanderthal skeleton, Boule, in collaboration with the neuroanatomist Raoul Anthony, had also provided the most detailed analysis up to that time of the Neanderthal brain, with the endocranial cast of the La Chapelle-aux-Saints fossil as the "type specimen." That work provided a whole generation of anthropologists with a way of discounting the most important piece of evidence that indicated the Neanderthals' near human status -- the large size of their brains as indicated by cranial capacity. As Osborn asserted and most of his contemporaries believed, "the absolute cubic capacity of the brain is less significant of intelligence than the relative development of those portions of the brain that are concerned in the higher processes of the mind."[79] Boule and Anthony had analyzed the general form and proportions of the La Chapelle brain and tried to trace the course of its major convolutions, all as indicated on the endocranial cast; they supposedly had discovered several marks of inferiority in that specimen, which they also claimed to see in other Neanderthal brains. Most of these "stigmata" were in what was then seen as the all-important frontal region. Since this region was generally thought of as the principal seat of the "higher" mental faculties, the fact that Neanderthal man appeared distinctly more "anthropoid" in the frontal lobe than in the Cro-magnon fossils studied by Boule and Anthony appeared to be proof positive that a great mental as well as physical gulf existed between the two "races."[80]

Just as in his handling of other fossils on the side branches of the human family tree, Osborn was inconsistent in his interpretation of the Neanderthal brain's supposed inferiority. On the one hand, as we have seen, he used it to underscore the lack of close evolutionary kinship between Neanderthals and Cro-Magnons. Yet in his general discussion of the evolution of the hominid brain, he used it along with the brains of Piltdown and Java man to illustrate the "stages" of development that the brain had gone through on its way to becoming fully human. As one might expect, the central feature of the process was the expansion of those parts of the brian concerned with the "higher" faculties of reason, foresight and language; in his graphic representation of this developmental series Neanderthal man was made to fit neatly between Piltdown and a "typical" Homo sapiens brain.[81]

By looking at the alleged inferiority of the Neanderthals, one really only grasps half of Osborn's rationalization for his race replacement scenario. Though they were less influential and representative, his views on the relative superiority of the Cro-Magnons were just as significant in justifying his position. In this part of the picture it was the glory of Upper Paleolithic art, or rather what it allegedly told about the racial psyche of its creators, that provided the main focus of attention. To Osborn the high level of artistic development attained by two Upper Paleolithic cultures -- the Aurignacian and the Magdalenian -- and the long process of evolution by which these heights had been reached bespoke the presence of what he termed a "unified art impulse." The unity of style and conception that could be seen in works in different genres such as objects of personal adornment, sculpture and especially wall paintings, could not have arisen, he believed, by a process of cultural diffusion and borrowing; it could have resulted only from an "inborn and creative urge," a unique psychological characteristic of a homogenous "racial type."[82] That this type itself had to be a "high" one followed from Osborn's awe at the level of cultural achievement reached, which indicated a greater "artistic sense and ability than any [other] uncivilized race which has ever been discovered."[83]

The "artistic sense" possessed by these Upper Paleolithic peoples more than any other character distinguished them from the Neanderthals, among whom, Osborn asserted, "no trace of artistic instinct whatever" had ever been found. Indeed, "prolonged study" of the former group's art had convinced him that they had possessed a capacity for "advanced education," a religious sense and a degree of "social differentiation" which in sum indicated a capacity for culture "nearly if not quite as high as our own."[84] The contast between such a race and the "brutish" Neanderthals could hardly have been more stark.

The race being extolled was of course the "Cro-Magnon," but whom exactly did he mean to raise to the status of "Paleolithic Greeks," as he often termed the group? The word "Cro-Magnon" had originally related only to the remains found at a particular site (as had the term "Neanderthal"). By the 1920s it seems, however, to have passed into the English language as a general term for the entire Upper Paleolithic population of <u>Homo sapiens.</u>

Osborn's usage fell between these two poles. For him it referred to the fossil remains associated with two Paleolithic cultures only, the Aurignacian and Magdalenian, remains which he claimed did belong morphologically to a single racial type. The type was best represented, he thought, by skeletons from the sites of Cro-Magnon and Les Eyzies in France, but it also included the "Aurignacian" man of Combe Capelle and the "Magdalenian" human remains found at Obercassel.[85]

That Osborn wanted to keep the association of specific cultures with specific races as strict as possible comes through clearly in his handling of two other issues relating to the Upper Paleolithic. The first had to do with the Solutrean "culture," which in the received unilinear scheme of cultural evolution came <u>between</u> the Aurignacian and the Magdalenian. Because the Solutrean contrasted with these two by virtue of its distinctive technique for producing stone points as well as of its diminished emphasis on decorative art, Henri Breuil had identified it as an import from the east which had been brought by an invading population. Osborn, agreeing with Breuil's assessment, tried to buttress it with the argument that the two sets of remains found in association with Solutrean tools -- at the sites of Brunn and Predmost in Czechoslovakia -- did indeed constitute a racial "type" distinct from the Cro-Magnons.[86]

Perhaps even more interesting was his handling of another "race," one that had sometimes been closely identified with the emergence of art in the Upper Paleolithic, the so-called Grimaldi race. The Grimaldi skeletons, which like other Upper Paleolithic forms clearly belonged to Homo sapiens, had come out of an Aurignacian deposit, but differed from the others, though, by their possession of several characters reminiscent of those encountered in modern Negroes or Bushmen. The idea that there could be either a "negroid" strain within or a "negroid" mixture with his "Paleolithic Greeks" was too much for Osborn. No, he asserted, the balance of characters in the Grimaldi race contained many features as "fine" as those possessed by the "most civilized whites" of today. The supposed Negro-like characters must only be analogies and not marks of true racial affinity[87] -- evidently because "fine" features could not survive the taint of Negro blood. Even thus purged, the group still did not qualify for inclusion in the charmed circle: Osborn felt the need to dismiss them entirely by arguing that since there was "no evidence of the survival of the Grimaldi Race -- we may safely attribute the entire art development to the Cro-Magnons."[88] The argument of course was circular -- it made sense only if one believed that the survival of the art required the survival of the artistic "race", which assumed the whole identification of race and culture that Osborn was attempting to prove by excluding the Grimaldi skeletons from the class "Paleolithic Greek."

As his discussion of the art of the Upper Paleolithic and the Neanderthal - Cro-Magnon transition made clear, Osborn was almost obsessed with a need to explain the cultural and morphological data relating to ancient hominids in terms of racial typologies. This preoccupation apparently blinded him to the important questions that had to be answered if a polyphyletic scheme of human evolution was to be firmly established. Was, for example, the splitting of the hominid line into divergent strands a case of "adaptive radiation"? If it was, what were the environmental conditions that had shaped the process of divergence? If it was not, then what mechanisms of non-adaptive evolutionary change should be invoked to explain the process? All these might be considered "theoretical" questions out of place in a "descriptive" work like Men of the Old Stone Age, but Osborn did not compartmentalize his thought in that way. If he left them out, it is because, I believe, he was concerned with portraying the later, Pleistocene stages of hominid evolution more as a human and thus "racial" phenomenon than as a problem in animal evolution. However, when he began to be even more concerned with focusing his scientific thinking on modern racial problems in the 1920s, he would examine both modern and fossil "races" in the light of such questions.

Tertiary Man and the English "Evidence"

In the years immediately following the publication of Men of the Old Stone Age Osborn did little to follow up the latter questions about polyphyletic evolution or any others on the topic of human emergence. The book had gone through two more editions by 1918, but given the fact that Europe, the center of prehistoric research at the time, was locked in total war, there appeared to be little to add to his previous conclusions. In the 1920s, however, partly because of the revival of research but also because of Osborn's increasing absorption in the general problem of how man fit into the evolutionary scheme, he began to develop a fresh viewpoint on human evolution. Though this "revised" version repeated several of the themes that had characterized Men of the Old Stone Age, it also contained new, and controversial, conclusions on some basic issues.

Though Osborn's new views developed over the course of several years, one theme remained paramount throughout his various writings -- his advocacy of the idea of "Tertiary man" ('Tertiary' was a conventional term for the geological eras of the Cenozoic, or Age of Mammals, prior to the Plesitocene). This concept of Tertiary man in turn broke down into two fundamental propositions: first, that contrary to previous theories, the genus Homo had appeared and had become well-established long before the onset of the Pleistocene; and second, that the close evolutionary kinship and relatively recent divergence between hominids and great apes often assumed by anthropologists were imaginary. When looked at as a whole, Osborn's attempts to demonstrate these propositions integrated several distinct strands of argument and apparent motive, strands which should be briefly described before any attempt is made to analyze them in detail.

Motives are difficult to pin down when one is dealing only with the published scientific record, but there are some clues, especially in Osborn's popular writings. Part of the impetus for the reevaluation of human ancestry seems to have come from a desire to make his science speak directly to some of the social issues of the time. Racial questions were matters of intense public concern in what John Higham has called the "tribal Twenties",[89] and the doctrine of "Tertiary man" could provide backing for his long-held belief in the importance and permanence of racial differences both in physique and mentality. The older mankind was, the longer, one could argue, had been the period of separate evolution among the various races and the greater the biological weight of racial differences.[90] The new theory could also be brought to bear on public debate over evolutionary theory itself. It became possible to defend evolution by jettisoning its greatest liability -- the notion of man's close kinship and recent emergence from the lowly "ape," and all that it implied.

That Osborn's concern with public questions became more intense in the 1920s was clear from his ardent support of the eugenics and immigration restriction movements and the involvement in the Scopes trial mentioned earlier. His rhetoric regarding the former issues became especially heated. The stresses of the period just following WWI - labor unrest, the apparent rise of 'Bolshevist' ideas in the radical community, the resumption of mass immigration from the poorer regions of southern and eastern Europe -- had obviously heightened Osborn's sense that the nation was at a critical point. As he told the delegates to the 1921 Second International Congress of Eugenics in his welcoming address, the eugenics movement was at that moment "engaged in a serious struggle to maintain our historic republican institutions through barring the entrance of those who are unfit to share the duties and responsibilities of our well-founded government."[91]

That the "unfit" included whole "races" as well as other typical targets of eugenics like the "feeble minded" Osborn made very explicit. He said in 1923, for example, that scientific data, in particular the Army intelligence tests done during WWI, had proven "many races and sub-races in Europe" to be "far inferior" to the native-born American of Anglo-Saxon racial stock.

In fact, physically and psychologically the "Nordic", "Alpine" and "Mediterranean" divisions of the Caucasian group differed so profoundly, he claimed in 1926, that "if we encountered them among birds or mammals we should certainly call them *species* rather than *races.*"[93] What was true among the Europeans was true *a fortiori* of the differences among whites, Mongoloids and Negroes which he called "absolutely distinct stocks that in zoology would be given the rank of species, if not of genera."[94] Osborn's animosity against blacks was particularly intense; for example, in 1923 he asserted categorically that "the negro's intelligence is not to be placed on the same line as that of the white man,"[95] and in 1926 that the average adult Negro was as intelligent as the average white 11 year old. Indeed remarks like these have caused one student of the eugenics movement to class Osborn as among the most racially prejudiced of the lot.[96]

Osborn was so unalterably committed to his views on race and immigration that any evolutionary scenario which lent them greater plausibility would have attracted him. In addition to these public concerns, however, there were a pair of more clearly scientific motives that showed through in Osborn's various elaborations of his new position. Several of the arguments he employed in defense of "Tertiary man" seemed to reflect a strengthened desire for an all-inclusive theory of the evolutionary process -- he seemed to feel that insufficient attention had been given by previous students to the requirement that human evolution conform to the "principles" that governed other animals. Two of these "principles" in particular, the "irreversibility" of evolution and "orthogenesis" -- the theory that the paths which evolution has taken are too regular to be explained by the selection of random mutations alone -- had in his view dramatic implications for the study of man. Another idea that he attempted to apply more rigorously to the evolution of mankind came from the field of mammalian evolution, i.e. the "Central Asian" theory of the origin of higher mammalian groups, which had received its classic statement from W.D. Matthew but its most vocal championing from Osborn.

The reasoning, assumptions and data behind the "central Asian theory" will be discussed below, but here it would be worthwhile to underline the importance that Osborn gave to it. So convinced was he that this region constituted the "paleontological Garden of Eden" that he lent the resources and prestige of the American Museum to no less than five major expeditions to Mongolia over the decade 1920-1930. These expeditions under the overall direction of Roy Chapman Andrews (1884-1960) included geologists, archeologists and paleontologists from the Museum's staff; in 1923 the President of the Museum himself, then in his 66th year, make the trip across the trackless wastes of the Gobi (in his own motorcar).[97]

In tandem the central Asian theory and Osborn's supposed "laws" of evolution could do a great deal to determine what the hypothetical Tertiary ancestors of man should look like and where they should be looked for in the future. What they could not do, in the short run at least, was provide convincing fossil and artifactual evidence of the presence of humans before the Pleistocene. Osborn was able, though, to scare up a small false alarm, which did not involve either a hominid or central Asia specifically but rather an alleged fossil anthropoid tooth from Nebraska, which Osborn called "Hesperopithecus." Had

"Hesperopithecus" panned out, it would have given the central Asian hypothesis a great boost, since under Osborn's scheme of mammalian distribution the plains of North America were an integral part of the "Holarctic" zone during much of the Tertiary. Unfortunately, the designation of "Hesperopithecus" as a primate turned out to be a hasty generalization.[98]

In the 1920s the major claims that evidence of Tertiary man had been turned up were coming from England. It seems only natural that with his great pride in his Anglo-Saxon heritage, his prestige in the scientific community, and the other motives mentioned above that were pushing him in the same direction, Osborn would become the foremost American defender of English Tertiary man. Once he had worked out that defense the specific characteristics of these Pliocene "Englishmen" would do much to confirm him in his other hypotheses. What was not the case, however, was that this new "evidence" had forced a reevaluation of his theoretical views. For while he dealt with the problem of Pliocene man in England before any full treatment of his new attitudes appeared, he was quite clearly predisposed toward an extension of human antiquity before he went to Europe in 1921, in order to examine the material on prehistory that had accumuated since his research for <u>Men of the Old Stone Age.</u> He confessed as much, when upon his return, he expressed the great pleasure that the new discoveries in England had given him, because they tended to support "his prophecy, made in April, 1921, before the National Academy of Sciences in Washington that one of the great surprises in store for us in science is the future discovery of Pliocene man with a large brain."[99] An examination of Osborn's English "evidence" will make clear the fact that the prophecy was in large part self-fulfilling.

The strongest part of the case for the presence of man in England during the Pliocene came not from fossils but from supposed artifacts. These had been found and described by the amateur English archeologist J. Reid Moir (1879-1944), who had conducted extensive studies of several geological deposits in the district of East Anglia. Reid Moir discovered flints of possible human workmanship at more than one location and geological level, but the find that was of critical importance for Osborn was the so-called "Foxhallian" industry.[100] These flints, which had got their name from the quarry in which they were first unearthed, were present there and in a deposit called the Norwich Crag, which supposedly placed the industry at the Plio-Pleistocene boundary. Reid Moir's later identification of flints of similar form at the so-called "sub-Red Crag" level in East Anglia appeared to place the industry much further back in time -- deep inside the Pliocene, however. At first, Osborn reported after his 1921 visit to the East Anglian sites, he had believed only some of the original Foxhall flints showed evidence of being true artifacts. By the time he returned to America he had changed his mind, partly because of first hand examination but also because the Abbe Breuil apparently had lent his support to the Foxhall industry, which on examination Osborn felt was continuous with that from the sub-Red Crag level.[101] Thus, he was now inclined to accept all of Reid Moir's material as human artifacts.

To Reid Moir, and now Osborn, the Foxhall "industry" was not a mere collection of "eoliths," stones which had marks of possible human use but no recognizable form. Rather, both believed that several distinct tool types were present, the most unusual being something called a "rostro-carinate," or keel-shaped scraper, which Osborn felt could have been used to separate animal hides from the underlying flesh.[102] The corollary of this thesis was easy to deduce. If a type of "Paleolithic" industry stretched back into the Pliocene, then human populations capable of a "high order of workmanship" must have existed earlier than previously thought.[103] Not logically necessary but also very tempting was the further inference that a longer Paleolithic meant a longer period for the preceding "Eolithic" age of hominid evolution prior to the emergence of the genus *Homo.* [104]

Accepting the authenticity and great age of the Foxhallian "industry" paid another important dividend as well in the search for Pliocene man -- it made possible a favorable re-evaluation of the Piltdown fossils. As one might guess, the major element of this new picture was a re-dating of Piltdown man -- back into the Pliocene. Osborn supported the change in date with two arguments. First, he decided that the flint "implements" found at Piltdown were not "Pre-Chellean" after all but rather "Foxhallian."[105] Second, he felt able clearly to identify two fragmentary elephant molars in the Piltdown gravels as belonging to the species *Elephas planifrons,* which he believed to be a widely distributed Pliocene species also present in the Red Crag deposits.[106] The implements and the "index fossils" thus corroborated each other and placed the Piltdown fossils firmly in the upper Pliocene.

As it turns out, Osborn had been deceived in both ends of his argument, and would have been guilty of hasty reasoning even if his evidence had been genuine. The Piltdown "paleoliths" were most likely waste flakes from a Neolithic stone tool workshop;[107] also, the remains of Elephas planifrons were not indigenous to the Piltdown deposit, but instead had been stained to match the gravels and salted into them by the perpetrator of the Piltdown forgery. In addition, according to J.S. Weiner (1915-1982), the British anthropologist who first researched the circumstances surrounding the hoax, Osborn erred in taking the presence of Elephas planifrons as indicative of an antiquity equal to that of the "Red Crag," since the latter deposit did not contain elephant fossils attributable to that species.[108] He was also on shaky ground logically when he saw the flint "implements" as independent confirmation of his presumed date -- the fact that these stones could be seen as "Pre-Chellean," "Foxhallian" or "Neolithic" depending on the context one placed them in shows how little a handful of crudely chipped flints can reveal on its own. Finally, there was the question of the "Foxhallian" culture itself, which the weight of archeological opinion has relegated to a place among the many "Eolithic" chimeras.[109]

Whatever weaknesses existed in Osborn's evidence and reasoning are less interesting, though, than the uses he made of his new "facts." He had gone to Europe at least partly in hope of finding Pliocene man. Reid Moir's material had excited his interest and strengthened these hopes, but as a biologist he apparently could not erect a new theory of human antiquity on the basis of artifacts alone.[110] His expertise in mammalian paleontology put him in a perfect position to provide crucial new support for the importance of the Piltdown fossils by calling them Pliocene in age. This "service" to English science would in turn reward him by providing him with what he had sought in going to England -- tangible evidence of Pliocene man. Once he believed he had that evidence, Osborn was apparently determined to extract the maximum value from it, for it became a central point in a full reinterpretation of hominid evolution, and especially of the evolution of brain and intelligence.

By the late 1920s, when Osborn began to promote his revised view of human evolution most actively, the critical importance of the Piltdown "brain cast" was clear. It appeared that Piltdown man had been a more capable creature than people had generally thought. The latter had, asserted Osborn,

> a surprising brain capacity of 1240 cubic centimeters. This brain cube equals that of the existing Indian Veddah tribes. As analyzed by Elliot Smith and Tilney, this Dawn Man has a well-convoluted forebrain, speech areas and diversified motor areas for the coordinated motions of the forelimbs, of the hands and of the fingers.[111]

These "facts," when taken in combination with several others -- 1) that some "cave men" (the Neanderthals) had been "inferior to ourselves neither in brain cube nor hand ability," 2) that others (the Cro-Magnons) had been "our superiors both in average brain capacity and in average artistic ability," and 3) that "the ratio of brain weight to body weight in Quaternary time was apperantly the same as it is today" -- in his view made a new scenario of human evolution "inevitable." It now seemed evident[112] that "the main cubic evolution of the human brain took place during the antecedent Tertiary time, and not, as we formerly thought, during the Quaternary Age of Man or Glacial Period."

Though this argument got him where he wanted to go -- an extended antiquity for levels of brain development comparable to those existing in modern _Homo sapiens_ -- it created some significant problems as well. First, there was one fossil that appeared anomalous -- the specimen of Java man found by Dubois. Not only did the fossil have a cranial capactiy of only 900-950 cc. but it had also been redated, by further studies of the Trinil deposits out of which it had come, as a Middle Pleistocene form.[113] In a way, the problem boiled down to one of simple choice: which fossil was more representative of the main line of human evolution, "Eoanthropus" or "Pithecanthopus." Today the choice is obvious -- Piltdown was a fraud, while Java man clearly belongs to the only Early to early Middle

Pleistocene human species yet found, Homo erectus.[114] In 1927, Osborn had grounds on which to choose differently. Though he did not discuss the fact since he wanted to make use of both fossils, the circumstances under which Dubois had operated made the exact identification of the date and associations of Java man seem as difficult as those of Piltdown.[115] In addition, for Piltdown he could point to the allegedly contemporaneous "Foxhallian" culture, while no artifacts had at that time been found in the Middle Pleistocene of Java. Thus, it was not surprising that he would see Piltdown as a full-fledged representative of "Upper Pliocene man," and as an "extremely adept flint worker, with deft hands and fingers guided by an imaginative and intelligent forebrain;"[116] on the other hand, the brain of "Pithecanthropus," he claimed, provided an example of "arrested development," and preserved a level of simplicity that the main line of human evolution had long left behind.[117] It seems clear that Osborn would almost have to have made the latter judgement, given the use he wanted to make of Piltdown. As we shall see, he was also able to rationalize it because it fit so easily into his central Asian theory of evolution.

The other major difficulty raised by Osborn's treatment of the Piltdown brain concerned his method of argument rather than his evaluation of various fossils. In raising the mental status of Piltdown man he had placed strong emphasis on the equivalence between its cranial capacity and that of members of existing human groups. To be sure, the objects of comparison were not civilized whites, but "Veddahs, Papuans and native Australians,"[118] all of whom ranked low in the racial hierarchy of white supremacists like Osborn. Still, the implication was clearly there -- that cranial capacity was a key index of level of intelligence. But if it were the key index, then the entire practice of reasoning from the details of endocranial casts to the intelligence of the creatures involved would become suspect. The latter practice, though, could not be so easily foregone, at least if one were commited to the biological inferiority of Neanderthal man, which Osborn had been since 1915 and continued to be. It was probably to protect this option that Osborn added the following qualification to his evaluation of Piltdown man: while the "brain cube" by itself was not a "reliable test of brain power or capacity," the existence of artifactual evidence could provide "collateral and very substantial proof that Upper Tertiary man ... made highly intelligent use of his 1240-1300 cubic brain measurement."[119] And in this case that evidence was provided by the discoveries of Reid

Moir.

Phylogenetic Principles and Primate History -- the "Pro-Dawn Man"

As the discussion above illustrates, the case for Tertiary man rested on a very narrow base of fossil and archeological evidence, so narrow in fact that the idea would probably not have attracted Osborn had he not had strong theoretical reasons for believing in it *a priori.* One major source of these reasons was his belief in certain "phylogenetic principles," which he himself asserted had originally led him to turn a personal hunch into a public prediction that evidence of Pliocene man would be found.[120] The way in which he expressed his understanding of these principles changed from year to year, as he adopted new categories and new terms to name them,[121] but the underlying meaning remained constant -- i.e. that the history of mammals revealed the insufficiency of theories that explained evolution in terms of merely passive or chance responses of organisms to changes in their environment.

While environmental, selective forces did determine the survival chances of existing species and the kind of organic change -- adaptive -- that had to occur, they could not explain the directions that changes in organic form would take, or the origins of new complexes of adaptive characters. Though the exact mechanisms by which the directions for existing patterns and the emergence of new patterns were built into the "germ plasm" (or "programmed into the DNA" as a later writer might have put it) were admittedly not clear to Osborn himself, he was quite sure that they existed and would be discovered.[122] Decades of research in vertebrate paleontology, he asserted, had convinced him that "not a single new organ is observed to arise fortuitously or indefinitely; it always arises gradually, continuously, and adaptively from its minute shadowy beginnings."[123]

The usual term for this directional change, or as Osborn once described it, "germinal potentiality of specialization along certain pre-determined directions rather than others in adaptive reactions to changes in environment,"[124] was orthogenesis. Once one was willing to accept the principle and decide on the directions of specialization that various major groupings had followed, orthogenesis provided a powerful tool for judging how closely specific animals were related to each other. It became more powerful for Osborn because of two additional principles that he applied along with it. The first, which followed jointly from orthogenesis and the idea of discrete adaptive patterns or complexes, was this -- that "every ancestral stage ... preserves the one hundred percent <u>structural equipment</u> for giving rise to its more recent or modernized descendants; each branch has the potentiality of the remotest twigs of descent."[125] The second half of the statement is a mere truism, but the first is not, for it seems to require that once a main line is established, all the members of that line will play out the adaptive possibilities inherent in the basic structures common to that line and add nothing new of their own. Of course, if the key phrase "structural equipment" were to be understood as "genetic potential," this would not be a necessary implication; the way Osborn applied the principle to the relationship between humans and apes, however, seems to have foreclosed this option. The second principle -- the "irreversibility of evolution" already discussed in the context of the <u>Age of Mammals</u> -- is really a corollary of the first: if a more ancient creature has specialized in a way that does not reflect fully the basic "structural equipment" possessed by a more recent animal, it cannot be an ancestor of that animal. Thus, Osborn argued, though "the evolution of functions and habits is frequently reversible," that of the specific structures involved could not be reversed.[126]

How, then, did these principles, which Osborn claimed had emerged from his work on other mammalian groups, apply to the evolution of the higher primates and to the problem of "Tertiary man" specifically? The central issue turned out to be the degree of relationship between man and the great apes. That this relationship was a close one had been a staple of evolutionary thinking since the time of Darwin, Huxley and Haeckel. Between 1910 and 1920 this viewpoint had definitely maintained its dominance among American scientists. The most respected expert on anthropoid anatomy in the English speaking world, Sir Arthur Keith, strongly defended the view that man had descended from a primitive form of "brachiating" ape.[127] The reigning U.S. authority on the evolution of primate dentition, William K. Gregory, had confirmed this opinion through his studies of the dryopithecines.[128] Osborn, invoking his phylogenetic principles and what he believed to be supporting evidence from the fields of comparative anatomy, embryology and animal behavior, opposed what he called the "ape-man" theory of Keith and Gregory, and the recent appearance of the hominids from the anthropoid stock that the theory implied. Instead, he argued that the great apes -- gibbons, orangs, gorillas and chimpanzees -- and humans represented the present end-points of two divergent lines of adaptation, each proceeding orthogenetically and irreversibly toward more specialized development of its basic structural, and psychological, equipment.[129]

Nearly all of the major structural differences that Osborn felt separated his two lines related to locomotion. Apes, he argued, all shared the same pattern of locomotion -- the "highly specialized arboreal type known as limb-swinging or brachiating."[130] On this he could agree with Keith *et al.* Comparative anatomy, he added, revealed that the specific characters which made for efficient brachiation were the following: 1) elongated forelimbs and a corresponding shortening of the hindlimbs; 2) in the hands, a shortened thumb combined with elongation and joint action of the other fingers, all leading to "the transformation of the hand into a hook;" and 3) the "transformation of the foot into a hand" by the lengthening of the digits and by the partial opposability of the big toe.[131] Man, by contrast, was marked by adaptation to bipedal locomotion on the ground. Human specializations for bipedalism, he argued, were exactly *opposite* to those required for effective brachiation: 1) a relative shortening of the arms and a corresponding lengthening of the legs; 2) a relatively long, fully opposable thumb and short, independently movable fingers which combine to make the manipulation of objects easier; and 3) enlargement and loss of opposability of the big toe and shortening of the other digits, making the foot more efficient in walking and running.[132] Given the principle of irreversibility, asserted Osborn, "we do not conceive it possible that the brachiating anthropoid could reverse the whole direction of its evolution, regain its lost powers and diminished organs and set out in an entirely new direction."[133]

Osborn also professed to find some support for rejecting an "ape-like" stage in human ancestry in the evidence provided by fossil hominid morphology and human embryology. The most ancient hominid limb bones then known, those of Java and Neanderthal man, showed only the short arm-long leg pattern of humans rather than the short leg-long arm pattern of the apes, a fact which indicated to Osborn that the former pattern was of long standing.[134] In addition, recent measurements of a series of human fetuses by Adolph Schultz (1891-1976), then at the John Hopkins University School of Medicine, showed hands with short fingers and relatively well-developed thumbs.[135] The embryonic big toe of humans likewise revealed "little vestige of former limb-grasping" as would have been expected had humans passed through a brachiating ancestral stage.[136] The embryological evidence made sense, however, only if one accepted another, implicit phylogenetic principle -- the so-called "biogenetic law" or principle of recapitulation, which said that the embryonic forms of descendants repeat, or recapitulate, characters possessed by adult forms of their ancestors.[137] The principle was by no means generally accepted at the time Osborn was writing; ironically, the source of his embryological evidence, Schultz, was himself a critic of the recapitulation theory.[138]

Problems like this did not trouble Osborn greatly. This was true, at least in part, because he felt that his own view was supported by a number of lines of argument that all seemed to reinforce each other. The locomotor differences between apes and humans that he perceived were associated in his view with psychological and ecological differences in adaptation that were no less fundamental. Because of their adaptation to an arboreal habitat the phylogenetic development of the ape brain had been "arrested." Escape from predators was easily obtained as were sources of food among the branches. The hands, adapted primarily for limb grasping, were used only secondarily for the manipulation of objects. All these characteristics added up to relatively little "stimulus" for further evolution of the brain, at least in contrast to the hominids.[139] The ancestors of man, living on the ground, were in a much less secure position than arboreal apes. "Vigilance, flight and concealment" were necessary to avoid the constant threat posed by large predators; in addition, the opportunity for an abundant food supply that existed on the ground promoted more effective use of the hominid forelimbs, which had been emancipated from locomotion. The means adopted by human ancestors to exploit this opportunity, "the adaptation of tools to certain purposes and needs of life, [and] the use of these tools," was the major factor, he believed, in the progressive enlargement of the hominid brain, which eventually dwarfed those of man's anthropoid relatives.[140] So unique was the final product -- the human brain, that in Osborn's view "no geologic period seems too long to allow for its natural evolution."[141]

A statement such as this might lead one to conclude that Osborn rejected a close relationship with apes because he wished to isolate humanity from the rest of the animal kingdom. Indeed, a remark that he made about his beloved Cro-Magnons in a popular work around this time -- that "the creation of this man of a higher order, with his moral, spiritual, and intellectual powers, is incomprehensible as purely a process of the survival of the fittest"[142] -- lends itself to such a view. Yet this conclusion would only be a partial truth. In rejecting the efficacy of natural selection he was leaving the way open for the role of orthogenesis; in attacking a recent ape-human divergence he was attempting to fit human phylogeny into a general pattern which he saw as normative for the evolution of the higher mammals. As we have seen, orthogenesis, the emergence of new adaptive complexes as units, and irreversibility were all part of this pattern.

While Osborn's version of orthogenesis and its application to human emergence were not anti-evolutionary (or even vitalistic, since Osborn did not believe the process to be driven by a nebulous "innate perfecting tendency" or a Bergsonian elan vital), their anti-Darwinian character must be emphasized. Like Cope, Osborn never ceased believing that the evolution of higher animals was too well-coordinated and complex a process to be accounted for by "chance" variations and "blind" natural selection.[143] As Peter J. Bowler has pointed out, paleontologists who adhered to orthogenesis viewed as "typical" evolutionary sequences those that seemed to represent long-continued, step by step intensification of a given morphological pattern. The lack of an extensively detailed fossil record in various groups often made the construction of such sequences easier, since there was less chance of encountering evolutionary side tracks. When the latter were found they would also be linked up in a trend of their own, producing the long parallel lines for which Osborn was famous.

Orthogenesis thus became a secularized, evolutionary successor to the Platonist, pre-evolutionary paleontology of such 19th century luminaries as Louis Agassiz and Richard Owen. Instead of various vertebrate fossils revealing progressive steps in the unfolding of a "divine idea," for Osborn the various descendants of a species would by hereditary mechanisms still unknown play out the structural potentials possessed by the founders of their line.[144]

This sort of evolutionary process seemed entirely compatible with a theistic belief in the "creativity" of the evolutionary process that Osborn seems to have required. In addition, it seemed to guarantee a gradual, strictly circumscribed brand of evolutionary change. Pedigrees would be of respectable length, and few creatures could be called "upstarts." In short, class boundaries were relatively impermeable in this version of the animal kingdom. In his study of the 19th century paleontological controversy between William Owen and T.H. Huxley, Adrian Desmond has hypothesized a connection between Owen's Platonist paleontology and his Tory politics; that Osborn was a political conservative and elitist as well as a believer in orthogenesis may not have been a coincidence.[145]

The theory of orthogenesis was the most general and perhaps the most important of the evolutionary principles that Osborn invoked against the "ape-man," but it was not the only one. In addition, he called upon certain theories concerning the roles of climate, habitat and geography in mammalian evolution which in his view also helped explain the early divergence of the hominid and anthropoid lines. In general, Osborn was convinced that the "separation of the adaptively radiating branches" in most families of mammals was "of an antiquity undreamt of" by earlier biologists. In the early Eocene, he asserted in a 1930 defense of his ideas on human evolution, hoofed animals like "horses, tapirs, rhinoceroses and titanotheres" had already "widely separated from each other in tooth, limb, hand and foot structure." More important, by the end of the Eocene these branches had split further into "forest-loving" and "plateau-loving" types, a process which by the succeeding Oligocene epoch had become "a sharp and worldwide division."[146]

The latter divergence in ecological adaptation had, he asserted, uniform structural and functional concomitants. The forest dwelling mammals had remained "backward, conservative types;" while on the plains and plateaus there had evolved the "alert, progressive, forward looking types, including all the long hind-limbed bipedal [quadrupedal?] animals adapted to rapid progression in an open or partly forested country." Given these facts, he asked rhetorically, was it probable that only the primates had escaped "this divorce between backward, forest-loving life and forward, plateau, savanna, and upland life, especially as Eocene forest areas in every continent began to contract and upland, open plains and plateaus began to expand?"[147]

If the general mammalian pattern did in fact fit the primates, to what instance of adaptive radiation among them would it apply more strongly than to the split between "forest-loving" apes and "ground dwelling" hominids? "The open country best adapted to the evolution of the horse" would also be best adapted to the evolution of man,[148] for "here alone" he asserted in 1929, "are rapidly moving quadrupedal and bipedal types evolved; here alone is there a premium on rapid observation, on alert and skillful avoidance of enemies; here alone could the ancestors of man find the materials and early acquire the art of fashioning flint and other tools."[149] Similarly, he argued in 1930, the timing of the ape-hominid split should match that of representative groups like the horse. It was in fact the discovery in 1919 of the Middle Pliocene *Pliohippus leidyanus,* "a perfect horse in all except name and perhaps color,"[150] which he said had led him to predict the future discovery of a "full-brained pro-man also in Pliocene time"[151] before the National Academy. With the backing of the English evidence for the latter proposition it seemed doubly reasonable to conclude[152] that

> when we at last discover one of our pro-human ancestors in Miocene or even in Oligocene time, the human characteristics will be found plainly stamped on this ancestor, as the horse characteristics are plainly stamped on the Pliohippus, on the Protohippus, on the Mesohippus and even on the Eohippus.

The conviction that even the earliest hominids would possess the "stamp" of basic human characteristics and would not be brachiators, no matter how primitive or unspecialized, led Osborn in 1928 to make the following hypothetical reconstruction of that earliest ancestor,[153] which he called the "Pro-Dawn man":

> The fingers of the ancestral hand were broad and separated, the thumb well-developed, with grasping power; the toes of the ancestral foot, on the contrary, were brought together, and the big toe was slightly separated. Thus in both the hand and foot these pro-human anthropoids were adapted both to tree and ground progression. Neither hand nor foot was so far specialized for extreme arboreal life as to be disabled for an early tool-making power of the hand and for a nearly bipedal cursorial power of the limbs or feet. Similarly, the pro-human brain conserved the alertness of all smaller primates in the terrestrio-arboreal stage but retained the potentiality of directing separate motions of the fingers and thumb in shaping defensive and offensive weapons.

The obvious criticism that could be brought against the "Pro-Dawn man" was that it was built up purely from the requirements of theory. No such creature had yet turned up in either Miocene or Oligocene strata. Osborn's reply, however, was also included in the theory -- the most likely place to look for the "Pro-Dawn man" had not yet been thoroughly searched. This place, of course was central Asia, specifically the "high plateau region of Asia embraced within the great prominences of Chinese Turkestan, of Tibet and of Mongolia."[154]

The Significance of Central Asia

As critics of the central Asian theory like Aleš Hrdlička and E.A. Hooton were quick to point out, the notion that Asia was the principal center of human emergence was a common idea that had not originated with Osborn.[155] Because the continent was vast, because it was the cradle of early civilization, and because its prehistory was still vague one could easily conceive of it as the source from which various ancient races had migrated into other, better known regions. Osborn himself had used it in this way in Men of the Old Stone Age when he portrayed Asia as the locus for the emergence of the Cro-Magnon race prior to its displacement of the

Neanderthals in Europe.[156] The central Asian variant of the _ex oriente lux_ hypothesis that he was now presenting, however, relied on a more purely biological basis than other formulations.

As was noted earlier, the central Asian theory of mammalian evolution represented a further development of the "Holarctic" theory of the distribution of major mammal groups that Osborn had presented in his 1910 work, the _Age of Mammals._ A critical stage in this further development had been provided by Osborn's colleague at the American Museum, William Diller Matthew in the 1915 work, _Climate and Evolution._[157] Extended discussion of Matthew's argument is not necessary here, since it proceeded from somewhat different theoretical assumptions than those Osborn held, and dealt with issues that are not directly relevant to Osborn's views on humankind. Nevertheless, on two key issues -- the differential effects of climate and the notion of a center of dispersal -- Osborn's arguments in the 1920s seem to presuppose those of Matthew.

Matthew's overall position on the biological impact of climate was quite straight forward and not at all novel -- namely, that life forms which evolve in response to cool, arid climates are more "progressive" than those which live under moist, tropical conditions. The reasons for this observation were as follows: first, a cool, arid climate tended to produce an environment "unfavorable to abundance of life," an environment which reduced the "ease with which animals could obtain a living."[158] This intensification of the struggle for existence supposedly came from the joint action of several "stimuli" -- "the inclemency of nature, the scarcity of food, the variations of temperature, as well as ... the competition of rivals and the attacks of enemies."[159] In addition to the stimulative effects of the cool climate, Matthew argued, there was the retarding effect of the tropical atmosphere, whose greater complement of carbonic acid and probably lower oxygen content would "tend to sluggishness" in the animals which breathed it.[160] Since the earth in its history as a whole had apparently alternated between phases of "warm, moist, tropical and uniform" climate and phases of "cold, arid, zonal" climate,[161] he concluded, "we should expect ... to find in the land life adapted to the arid climatic phase a greater activity and higher development of life."[162]

If the argument had remained here, of course, central Asia would never have assumed the great importance that it had for both Matthew and Osborn. Somehow one had to move from worldwide climatic changes to a localized "center of dispersal." Matthew did this with two simple propositions -- 1) that during "progressive" phases the environmental causes of evolution will act in some single region with "maximum force," and 2) that "so long as the evolution is progressing steadily in one direction, we should expect them [environmental causes] to continue to act with maximum force in that region."[163] For Matthew, the prime candidate for this region was central Asia. The general rationale for this choice was the following: the present distribution of continents, which in Matthew's view had been pretty stable throughout the geological past, indicated that cooling phases would act first, with the greatest impact, and upon the widest area in the interior of the "great northerly masses" of America and Asia. Since the interior of Asia had the more strategic location over the course of time, it was this part of the "Holarctic" region that would have acted as the principal center for the evolution of higher mammalian forms. Thus, Matthew envisioned successive waves of advanced forms arising in central Asia and migrating outward; "the tropical and southern continents would be the refuge of the less adaptable and progressive types," which could no longer hold their own in the regions closer to the center.[164]

Matthew did not rely only on a general discussion of climatic and geological history to substantiate his theories. Indeed, most of Climate and Evolution was taken up by an examination of past and present patterns of animal distribution in order to determine whether or not they matched theoretical expectations. Interestingly, while Matthew did try to integrate the living races of _Homo sapiens_ into his scheme,[165] he said very little about the earlier phases of hominid evolution. But though Osborn had not been anticipated in his attempt to apply Matthew's ideas to early humans by Matthew himself, that had been done, as Osborn was willing to confess,[166] by the Yale geologist Joseph Barrell in 1917.

Barrell's theory is interesting both for the degree to which it anticipated Osborn's argument and for the ways in which it differed. Barrell, like Osborn, asserted that adaptation to a north temperate climate in the grasslands of central Asia had brought about the evolution of the first hominids from a previously arboreal but not overspecialized primate stock. Likewise, he believed that this change depended on a group of closely correlated adaptive characters, though his list was a bit different from the one Osborn used in distinguishing hominids from apes. Barrell's list (which clearly derived from William K. Gregory's 1916 work _Studies on the Evolution of the Primates_) included changes in foot structure, limb proportions and curvature of the backbone that aided bipedal locomotion, a shortened jaw and changed dentition that allowed a shift from plant foods to an omnivorous-carnivorous diet, and development of the brain for enhanced "mentality" and social cooperation.[167]

Though these points of agreement were important, Barrell differed from Osborn in several critical areas. First of all, he rejected the possibility that the structural transformation that produced the early hominids might have been determined from within. Though he admitted the existence of orthogenetic trends, he could not see how the hominid complex of interrelated adaptations could have been so produced: "the law of probabilities," he said, "declares that such a new and efficient combination of organs could not arise by spontaneous and orthogenetic variations in each unrelated part."[168] To Barrell "strenuous selection" over a long period of time seemed the answer.[169] In addition, Barrell did not think the human line went as far back as the Oligocene, but rather had emerged in the succeeding period, the Miocene; during this period there had occurred not only a shift from forest to grassland conditions in many parts of Eurasia, but also the uplifting of the Himalayas, which had placed a formidable barrier between the plateau regions of central Asia and forest regions to the south.[170]

The reason why this last fact was important to

Barrell raises the key distinction between his scheme and Osborn's; Barrell supported the "ape-man" theory. He also supported a Miocene date for the first hominids because the geology seemed right and because he felt with Gregory that a "generalized" dryopithecine made a good starting point for the human line. Dryopithecine fossils had been found in the Miocene and Pliocene "forest faunas" of the Siwaliks, a range of hills on the southern side of the Himalayas. What if a generalized species of this group had become "stranded" on the central Asian plateau while the climate gradually became cooler and the vegetation slowly shifted from forest to open plain? Conditions would seem to have provided both the "strenuous selection" and the geographic isolation necessary to produce a form of early human creature.[171] Thus, while Barrell agreed with Osborn that the search for the earliest hominids should occur "in deposits of the open and temperate regions of central Asia," the former would be looking for a "Miocene ape-man" and not an Oligocene "Pro-Dawn man."[172]

The foregoing look at Barrell clearly indicates that Osborn's particular theory of hominid emergence was not a necessary consequence of the central Asian idea. What then accounts for the specific variant of the latter theory represented by Osborn's "Pro-Dawn man"? To a large degree, he could not accept a view like Barrell's because he had come to believe that it violated his conception of the evolutionary process in general; hominids and apes, however primitive and generalized, expressed different basic patterns of adaptation. Creatures that bore the "stamp" of either pattern could evolve only by developing some of the adaptive possibilities inherent in that pattern, and not by "jumping the track," as it were. But also the divergence in result came from the fact that each man was using a different aspect of the theory developed in Climate and Evolution as his starting point. For Barrell it was the idea of climatic phases and how a period of transition might bring about a major evolutionary re-direction in the primate line. For Osborn, as indeed it had been for Matthew, the central issue was the more general one of how "progressive" forms had arisen, again and again, in the ideal environment of the central Asian plateau.

Whenever Osborn described central Asia, he portrayed it as a veritable volcano spewing forth new life-forms. In 1924, while summarizing the work of the American Museum's expeditions to the region he gave it the glowing title of "this great home-country of land reptiles and of land mammals."[173] In trying to justify the title he echoed the reasoning of Matthew -- central Asia was the preeminent example of "an elevated country of the savannah type, largely open, partly forested, in which there was throughout a severe competition and struggle for existence leading to highly varied adaptive radiation."[174] And as we have seen, this reasoning was central to the anti-"ape-man" argument of the later 1920s. That it was present almost from the start of his reevaluation of human ancestry emerges from an address he made in Peking after he had gone with the 1923 expedition to Mongolia. "An alert race," he declared, "cannot develop in a forest -- a forested country can never be a center of radiation for man ... It is upon the plateaus and relatively level uplands that life is most exacting and response to stimulus most beneficial ... In the uplands of Mongolia the conditions of life were apparently ideal for the development of early man."[175]

As it turned out, Osborn was so convinced of the role of the central Asian uplands in the development of humanity that he invoked the theory not only in regard to the "Pro-Dawn man" but in order to explain the later stages of human evolution as well. It had been a major weakness in the previous polyphyletic scheme outlined in Men of the Old Stone Age that there was no clear adaptational basis for placing various fossil hominids on side branches. This criticism applied especially to the

Neanderthal - Cro-Magnon transition -- while Osborn had invoked "degeneration" and the inability of the Neanderthals to meet the stresses of the last glaciation, he had really not been able to account for the faster rate of evolution which gave the Cro-Magnons their superiority. As one might expect, in the 1920s the key to the problem became the "forest-loving" vs. "plateau-loving" distinction that he had employed with regard to hominids and pongids.

The solution became apparent to Osborn once one grouped the various "Neanderthaloid" races, and examined their area of distribution. Physically, "Neanderthaloids" could be defined as all the "prehistoric races with prominently projecting supraorbital processes," low, sloping foreheads and a matching "low, broad type of brain, especially with low forebrain."[176] The "cultures" associated with the "Neanderthaloids" comprised, he asserted in 1927, all the Early and Middle Paleolithic industries found in Europe -- "Cromerian, pre-Chellean, Chellean, Acheulian and, finally, Mousterian."[177] If the sites where either the "Neanderthaloid" fossils or cultures had been found were mapped out, it would show that the "Neanderthaloids" had "apparently dominated North Africa and all of Europe and extended eastward into the heart of Asia" for most of the

Pleistocene.[178] This wide range was further underlined by recent discoveries -- the Rhodesian skull, with its massive supraorbital ridges and low forehead, and the Mousterian camp site at Ordos, in China.[179]

Now, Osborn claimed, the fauna "contemporaneous with this race" was well-known. The large mammals usually included elephants, "especially of the southern and straight-tusked types, rhinoceroses and, in the lower lands, hippopotami."[180] This "South Temperate fauna" was, not surprisingly, adapted to "rather fertile lands, river bottoms and abundant forests [my emphasis]."[181] The ecological consequences for the hominid population were easy to draw:

> In such an environment game was so plentiful that there was relatively little struggle for existence, hence there was little incentive to the development of a diversified flint industry. Superior intelligence was not demanded and it is therefore surprising that under these circumtances the Neanderthal brain attained the dimensions which threw even the genius of Huxley off the track as to the very primitive character of this race.[182]

In a context like this, while the size of the Neanderthal brain might be surprising, the inferiority of that brain in relation to that of the "higher race" that succeeded the Neanderthals no longer was any surprise.

Still, half of the problem remained -- where had the invaders who eventually displaced the "Neanderthaloid" race come from? Where was "this unexplored territory, the unknown homeland of the higher races of man."[183] It could not, of course, be to the south -- for there life was even easier, and provided less "stimulus" for the development of intelligence. In southern Asia conditions were easy enough that they might have allowed the primitive "Trinil race of Java" to persist long after the hominids of other regions had evolved higher brains. Africa was also unlikely, though Osborn could envision the possibility that the "Negroid stock" had ememrged "under Central African conditions that must closely parallel those of Central and Southern Eurasia during the great Neanderthal period."[184]

The answer thus was inescapable from Osborn's perspective -- one had to look "to the northern regions of Eurasia ... to a temperate and north temperate region which extended along the northern borders of the Neanderthal empire over the high central plateau region of Asia, over the great plains region to the north of the central plateau and, finally, over the confines of eastern Europe."[185] There, where the struggle for existence had been "much more severe," had evolved the faculties which gave the "new modern races ... physical and intellectual supremacy over the Neanderthals."[186] Not only was he sure that modern humans had developed in that plains and plateau environment, but he was also convinced that an immense span of time had been necessary to produce this "very high modern brain power." Look, he said, at the "astonishing industry and diversified art" of the Cro-Magnons and consider that each of mental abilities required to produce them had its own "cerebral equivalent and ancestry." Abilities like the "extreme accuracy in the depiction of animal form displayed by the Cro-Magnons" were not the product "of hundreds of years, but of hundreds of thousands of years."[187] Only polyphyletic evolution, with the "progressive" races occupying the ideally "stimulating" environment seemed to explain the emergence of Homo sapiens.

Problems Implicit in Osborn's Later Views

In weaving together the central Asian theory, his "phylogenetic principles," his belief in Pliocene Englishmen, and his multilinear theory of Pleistocene man, Osborn seemed to have produced an attractive fabric of argument. The identification of prehistoric races with prehistoric cultures, the notion of race replacement, the glory of the Cro-Magnons[188] and the inferiority of the Neanderthals all appeared more secure than in 1915. Finally, too, the "ape-man theory" could be "banished from our speculations and from our literature not on sentimental grounds but on purely scientific grounds."[189] The key differences in structural and psychological adaptation that he had discerned between humans and apes could now be seen as the essential evolutionary facts; the many similarities adduced by Keith, Gregory, et al. were either joint inheritances from more primitive primates or instances of "convergence."[190]

Of course the ability to explain certain facts and interweave them does not guarantee that one's explanations are true, that they convince colleagues, or that they provide a stimulus for further research and discovery. While Osborn's theories about central Asia and the various "Dawn" and "pro-Dawn" men received their share of public attention and scientific discussion,[191] they were generally not accepted by other experts on human evolution. Though the details of specific scientists' attacks, especially those of William K. Gregory, will be looked at in other chapters, and major failings that colleagues discerned can be summarized here. In addition, some mention needs to be made of problems that were not noticed at the time, but reveal patterns of thought characteristic of Osborn.

The most obvious problem that Osborn had to contend with was a lack of fossil evidence -- neither a "Pro-Dawn man" nor a "high-browed" Pleistocene ancestor of Homo sapiens ever turned up in central Asia. Between 1920 and 1930 Roy Chapman Andrews led several American Museum sponsored expeditions into the Mongolian wilds. They uncovered some exciting fossils, including nests of dinosaur eggs and unusual "shovel-tusked" forms of mastodon; there were even some apparently paleolithic artifacts,[192] but there was no physical evidence revealing the morphology of the toolmakers. Of pre-Pleistocene hominids nothing at all was found. Though these facts provided some comfort for the sceptics they could not really shake the faith of true believers -- hominid fossils were notoriously difficult to find, after all. Also, the reconnaissance of promising deposits had ended in the early 1930s not because the major sites were exhausted but because political approval for the expeditions was withdrawn by the Nationalist government. Thus it became possible to argue that only a chance to complete the work begun earlier stood in the way of Osborn's full vindication.[193]

Fossils aside, Osborn ran into difficulties because the strong distinction between humans and apes in both form and behavior that he was promoting really went against the dominant trends of research in comparative anatomy and psychology of the primates in the 1920s.

Thus, while Osborn was trying to force all the pongids into a uniform, "brachiating" pattern of limb form and proportion and all men into a "bipedal" pattern, Adolph H. Schultz was stresing the _variability_ in these characters among the pongid species, and the close resemblances to humans that could be found in one ape or another.[194] In contrast to the rigid characterization of apes as a group structurally confined to "arboreal" life, Dudley J. Morton's anatomical studies of the primate foot showed gorillas to have several characters that made for improved ground mobility vs. other pongids[195] Also, as William K. Gregory argued against Osborn and others, if one looked at the anatomy _as a whole,_ the apparent homologies between hominids and pongids were so numerous that a relatively recent common ancestor for both groups was still the most parsimonious hypothesis.[196] Finally, when Osborn judged apes to have a conservative, "forest-loving" psychology and humans a progressive "plains-loving" one, he was apparently ignoring the implications of the best-known experiments on ape behavior -- those of Robert M. Yerkes (1876-1956) and Wolfgang Köhler (1887-1967), which showed a great degree of kinship between humans and the great apes, especially the chimpanzee, in key aspects of behavior like problem solving, manual dexterity and even tool-use.[197]

If Osborn could be accused of being too selective in his use of the existing evidence on the higher primates, he could also be criticized for the broad and speculative theories he advanced. The most obvious problem was the central Asian idea itself. It was not at all clear to Osborn's critics why there had to be a single center for human evolution; not only was fossil evidence lacking that would show central Asia as the center of primate evolution,[198] but Osborn himself had noted some other mammal groups that were exceptions to his rule, such as the proboscideans, or elephant family, of apparently African origin.[199] In addition, why was it, Ales Hrdlicka asked, that the center of evolution for a polyphyletic theorist always had to be a place where hominid fossils so far were absent from the geological record?[200]

The central Asian theory was also weak because the critical ecological distinctions that it made -- between forest and plain, between tropical zones and temperate zones, though plausible in their day, did not rest on strong evidence. In regard to fossil hominids, Osborn undertook no detailed analysis of individual sites to demonstrate that his "Neanderthaloids" had only lived in "rather fertile lands, river bottoms and abundant forests." Also, if the "high-browed" Pliocene "Dawn men" of England had been closer to the main, plains-adapted line of human evolution than the early "Neanderthaloids" -- as Osborn implied they were,[201] they must have been more "progressive" in both physique and mentality. If so, why had they disappeared from Europe, to be replaced by an apparently "inferior" stock? In regard to the theory generally, the idea that plains life was more "stimulative" and productive of "progressive" species was not an obvious truth. It would seem that the relative pressure of the "struggle for existence" in various environments depends very much on the kind of "structural equipment" an animal starts with. What might be true for ungulates might not apply to primates. Indeed, in the 1920s the most generally accepted account of primate brain evolution was still that of Grafton Elliot Smith, who stresssed the selective pressures that arboreal life put on hand-eye coordination, planned movements and other forms of behavior that required increased brain power.[202]

Even if the transition to ground living could be seen as a critical change that forced a new burst of rapid brain development in early hominids,[203] it would still require some proof that a terrestrial way of life in tropical forests really was easier and less stimulating than life on the cool upland plains once the hominid adaptive pattern had become established. The necessary evidence would today be sought via the sub-discipline of paleoecology, in which the fauna associated with human fossils would be subjected to varous kinds of quantitative analysis. To say that these tools were unavailable to Osborn's generation, while true, begs a fundamental question -- why did Osborn choose to advance major generalizations so confidently without even trying to approximate such techniques? In large part it must have been because his climatic theory appeared almost obvious, and it appeared so because it was analogous to deeply rooted attitudes about the present races of mankind that Osborn shared with many of his contemporaries.

It had long been a staple of racist and colonialist thought that the tropics did not conduce to high intellectual achievement or cultural progress in the races that inhabited them. Early evolutionary thought had strengthened this belief by giving it a secure biological basis -- tropical races were inferior because they had evolved in adaptation to a less stimulating or even debilitating environment.[204] That these attitudes were still flourishing around 1920 can be seen in the books written by the noted Yale geographer Ellsworth Huntington, who claimed to have identified, through correlation of various civilizations with historical data on climate, an optimal climate for human mental performance. Not surprisingly, the ideal was a cool, temperate enviroment -- with a mean of 30-45 degrees Fahrenheit for January, and 65-75 degrees for July, and one which provided a significant amount of stress due to variable atmospheric conditions like storms.[205] Huntington also argued that the races differed in intellectual capacity according to the climatic forces which had shaped them.[206] Matthew had employed similar ideas as well in the discussion of man he included in Climate and Evolution.[207]

That Osborn fully accepted both principles in regard to modern races can be inferred from the way he dealt with the hominid fossil record but it can also be seen directly in statements he made. In the period of the "genesis of human races", he argued in 1926,

> man goes forth to seek and labor for food. He may go the temperate regions, the North Pole, or to the Equator. If he chooses the Equator the quest for food is very easy and requires relatively little intelligence; the environment is not conducive to rapid or varied organic selection; the struggle for mere existence is not very keen; the social and tribal evolution is very slow; intellectual and spiritual development is at a standstill. Here we have the environmental conditions which have kept many branches of the Negroid race in a state of arrested development.[208]

The varied pressures of the universal "quest for food", when combined with the great age of Homo sapiens, had, according to Osborn, resulted in profound differences between the major racial groups, differences that he felt were equal to those among various animal species. That such great differences could arise testified, he asserted, to the power of "adaptive radiation" over both animals and humans.[209] It seems just as likely, though, that only Osborn's strong assumption that racial differences in capacity were deep, heritable, and permanent allowed him to apply the concept of "adaptive radiation" at all. As in so many cases from the period,[210] social prejudices probably played a key role in the choice of the biological principles, including "phylogenetic" ones, that one would apply to humankind.

There were weaknesses in Osborn's other "phylogenetic principles" as well. Leaving aside the question of how adaptive complexes of characters first appear, the notion that an ancestor must have the entire "structural equipment" of its descendants has to be limited in its application; otherwise new forms of adaptation would never occur unless they could utilize old "equipment." Osborn could accept such limitations, apparently, before the emergence of the major mammalian groups but not after. Why this was so was not clear. Also, the way he used the notion of irreversibility was made unnecessarily strict by his adherence to orthogenesis. Even if one accepted the "100%" theory it would be possible to produce many of the adaptive structures of humans from those of the great apes through simple changes of proportion. As Gregory pointed out, only the idea that such changes had to occur in only one direction over time allowed one to use the principle of irreversibility against the "ape-man," and it was an idea that other sequences in the evolution of mammals appeared to contradict.[211]

Taking all these weaknesses together, one could legitimately ask whether Osborn's much repeated speculations had anything to teach us other than the power of theory to overpower evidence. After all, his morphological arguments were often superficial, he lacked fossil evidence for most of his major conclusions, and his interpretation of archeological data was strained. Also, he had advanced an extravagant polyphyletic theory of primate evolution, in which apes and hominids had evolved separately, and often in parallel, from Oligocene times on, and the main human races had done likewise since the early Pleistocene, if not earlier. If one were to subscribe to a "Whig" theory of the history of ideas, one might conclude that Osborn's later work on primate evolution was of limited interest indeed.

Of course, there are other ways to assess his contribution, ways that appear to justify the effort to describe and analyze his work in some detail. First, the issues he raised, if not his specific positions on those issues, did prove fruitful by stimulating debate and further research. Osborn's attack on the "ape-man" theory not only helped provoke attempts by more orthodox thinkers like W.K. Gregory and E.A. Hooton to defend man's brachiating ancestry,[212] but also, in combination with the latter, provided the background for the work of anatomists who were sceptical of the evidence for both "dawn men" and "ape-men".[213] Indirectly, Osborn's later theories also had some influence on the reception accorded to one of the most important discoveries in the field of human evolution -- that of Peking man.

That influence was indirect, though, for several reasons. First, though the motive force behind the excavations at Choukoutien that produced the "Sinanthropus" population was the belief in the central Asian theory of human evolution, that belief was represented in the person of the Canadian scientist, Davidson Black (1884-1934), who was neither a follower nor a student of Osborn. Rather, Black had adopted the theory directly from Matthew, a fellow Canadian, and took up a professorship in anatomy at Peking Union Medical College in 1919, fully committed to the idea that important fossil hominid remains would be found in China.[214] This also was well before Osborn began championing the theory on his own; but Black did not publish the major theoretical statement of his belief until 1925,[215] _after_ Osborn had made central Asia a "hot" scientific topic with his much publicized expeditions to Mongolia. The first specimen of Peking man -- a single tooth -- was not unearthed by Black, but Black's description of it as representative of a new _genus_ of fossil man, _Sinanthropus pekinensis,_ in 1927 caused great controversy. Black then undertook a concerted excavation of the Choukoutien deposit, which in 1928 revealed skull and jaw fragments confirming Peking man's hominid status.

While the American Museum's central Asian expeditions had turned up nothing, Black had ironically made one of the finds of the century working on similar theoretical assumptions. Black and Osborn seem also, from the above account, to have been working in parallel, with only the coincidence of timing to connect them. But another major connection emerges when one considers the fact that Black's researches were financed by an American organization based in New York -- the Rockefeller Foundation.[216] While only detailed research into how that organization made its funding decision would confirm it, it seems reasonable to hypothesize that the prestige which Osborn lent to the central Asian theory and the prominent place that he had given it in the debate on human origins had to have been factors in the Foundation's willingness to back up Black's somewhat hasty conclusions with hard cash.

There was another, ironic connection between Osborn and Peking man, but this one operated in the reverse direction and after a lapse of several years. At first, it might have been possible to classify "Sinanthropus" as merely another Pleistocene side branch of the human family that was too "primitive" to have contributed genetically to Homo sapiens (as Osborn apparently did),[217] or as a "Neanderthal in the making" (the option chosen by E.A. Hooton).[218] Eventually, though, the large number of individuals and the well-documented similarity of "Sinanthropus" to another Middle Pleistocene form, "Pithecanthropus," almost had to stimulate re-evaluations of the idea of large-brained Pliocene Homo and even that of polyphyletic evolution itself. The primary agent of the reevaluation was the noted German anatomist, Franz Weidenreich (1973-1948), who succeeded Black after the latter's death in 1934. Weidenreich's monophyletic view of human evolution had a great impact on American scientists -- for he had not only provided the definitive descriptions of Peking man, but had used the occasion to undertake a full re-examination of fossil hominids in general. The resulting writings, especially the classic monograph The Skull of Sinanthropus Pekinensis,[219] received a respectful hearing in America, especially since their author had left China with the onset of WW II and come to the U.S. to continue his work under the auspices of the American Museum of Natural History.[220] After Weidenreich's contibutions it would become very difficult to ignore the "low-browed" Middle Pleistocene hominids who came to be known as Homo erectus, and look to hypothetical Pliocene creatures for the ancestors of modern humans.

To say that a scientist's work has helped stimulate the efforts of those who would make his ideas obsolete is of course a backhanded way to show his lasting influence and importance, and one that Osborn himself would probably not have reveled in. Importance can be gauged in other ways than in the degree of influence on later theories. One way is by providing an example of a fruitful procedure, even if the conclusions one reaches prove unconvincing. Here Osborn truly was significant, for he was showing a way to break out of a pattern of research that was inhibiting fresh approaches to the problem of human evolution. It was one of the great problems of physical anthropology prior to World War II, and a problem often noted during the postwar period,[221] that research tended to focus on descriptive biometric and morphological studies, and to neglect the study of function and adaptation, as well as the application of general evolutionary theory to the case of human beings. Osborn's "phylogenetic principles" and his use of evidence would hardly have fulfilled postwar ideals of soundness. Still, he was moving in a useful direction, for he was proposing a reexamination of the data on primates and fossil hominids in the light of general evolutionary principles, in order to see if orthodox conceptions of human evolution might have to be changed. In the context of this approach, he was raising questions about patterns of adaptation, and how they evolved in relation to their environment, that needed to be injected into the discussion of human evolution.[222]

The final way in which one can see Osborn's significance is to recognize that many of his ideas were representative of the climate of thought about human evolution between 1910 and 1930. The later stages of the present discussion have emphasized the more controversial aspects of Osborn's post-1920 writings, but some aspects of these writings continued his earlier themes and reflected a broad consensus among his contemporaries regarding the later phases of the human evolutionary career. The most important of these ideas are the following: 1) the ease with which racial "types" were defined on the basis of single, often incomplete specimens, 2) the habit of invoking racial competition as a means of evolutionary change, 3) the identification of cultures with the "races" that supposedly produced them, and 4) the conviction that parallel phyla of hominid evolution for most of the Pleistocene best explained the existing fossil and archeological data. Specifically, the last idea was almost normative for discussions of the transition between the Middle and Upper Paleolithic, i.e. that between the "Neanderthal" and early modern _Homo sapiens,_ populations since the two transitions were considered equivalent. It cannot be said that he originated any of these ideas, but one can argue that Osborn played a major role in disseminating them -- through popular works like _Men of the Old Stone Age_ and the 1927 book _Man Rises to Parnassus,_ through the attention the press gave to his views, and through museum exhibits like the "Hall of the Age of Man" at the American Museum.

Even the more extreme aspects of Osborn's later views can be seen in part as an attempt to build a convincing evolutionary scenario around the commonly held phylogenetic ideas summarized above. That he exceeded most of his contemporaries in the degree of distance he put between humans and great apes, in the rigidity of his orthogenetic views, and in the racial animus he displayed is clear. This very exrravagance should aid us, however, in detecting the presence of similar themes in the writings of other scientists at the time, especially when these themes were vaguely sketched in, or implicitly assumed, rather than boldly proclaimed. In addition, Osborn's more extreme biological arguments made some interesting connections with the social ideology he espoused so vehemently in the early 1920s. They thus provide an illustration of how social concerns can contribute to the shaping of biological thought, a phenomenon which can be seen, in a more muted form, in other writers such as E.A. Hooton.

There was another, and somewhat puzzling, characteristic of Osborn's view of fossil hominids that was also representative of the times -- i.e. the way it tended to foster a conception of prehistoric populations that worked against the general evolutionary approach he appeared to be promoting. Partly this "non-evolutionary" bias resulted from the so-called "ancestorless man" phenomenon,[223] in which the best known fossils were judged too primitive to be included in the direct line of human descent. This, in turn, was closely related to an attitude of which Osborn was the prime American exponent

-- one which combined great respect for the capacities of Upper Paleolithic peoples and devaluation of the physical and mental advancement of their European predecessors, the Neanderthals. Indeed, Osborn once said that he was "more proud of having helped to redeem the character of the caveman than of any other single achievement of mine in the field of anthropology," and then made it clear he was speaking not of "the extremely ancient order" of caveman, the Neanderthals, but of "the Cro-Magnon race of artists."[224]

The purpose of this redemptive aspect of his work, he asserted elsewhere, had been to cleanse the caveman "of his reputation of being very close to the brutes,"[225] at least those cavemen he considered ancestral to white Europeans. To try at one time both to conceive of human emergence as an instance of animal evolution and to put an evolutionary gulf betwen Homo sapiens and his best known "brutish" predecessors was a clearly schizophrenic undertaking. And maintaining a consistently evolutionary approach became doubly difficult because the easy resort to static, racial explanations of fossil hominid relationships diverted attention from the problem of evolutionary transformation.

To be sure, not all of the major writers on fossil man between 1910 and 1940 shared Osborn's difficulties completely. Nevertheless, either the "schizophrenic" quality or the general habits in interpreting later hominid evolution noted above were present in all of them to some extent. The fact that Henry Fairfield Osborn was the oldest of the group, as well as the writer whose work presents these problems in the most extreme form, makes him an ideal starting point for the analysis of the others.

CHAPTER II

GEORGE GRANT MACCURDY, 1863 - 1947

A Brief Account of MacCurdy's Career

George Grant MacCurdy was a pioneer in the field of Paleolithic archeology in the United States, serving for a quarter of a century as Professor of Archeology at Yale University and curator of the anthropological collections at Yale's Peabody Museum. A respected and prolific author in his field of expertise, which included the morphology of fossil hominids as well as prehistoric archeology, MacCurdy was also a founding member of the American Anthropological Association and the motivating force behind the American School of Prehistoric Research. His combined achievements caused one of his biographers to call MacCurdy "the leading exponent and authority outside Europe" in the field of Paleolithic archeology during his era, and another to note that "no scholar outside the Old World" had made "so many notable contributions to the study of its prehistory."[1]

The facts of MacCurdy's background and youth carry the distinctive savor of America at the beginning of the industrial era. He was born in 1863 in Warrensburg, Missouri, the son of an ex-planter from Georgia who had freed his slaves because of his opposition to the institution and had moved westward with his family. The MacCurdy family apparently did not prosper in its new location, and in his youth George Grant MacCurdy decided to leave the farming life behind. As it must have for many other young men in small town and rural America, the profession of public school teaching provided the first step outward. In 1881 MacCurdy enrolled at the State Normal College at Warrensburg. Progress was slow because MacCurdy had to alternate attendance at college with the series of teaching jobs which earned his tuition. After graduation in 1887, however, advancement came quickly; two years later the youthful MacCurdy was appointed superintendant of schools in his home town.[2]

A crucial turning point came in 1889. A YMCA conference at Mt. Hermon, Massachusetts, gave MacCurdy his first opportunity to visit the area of Boston and Cambridge; that visit apparently kindled in him a strong desire to attend Harvard University. In 1891, he was able to obtain a scholarship and was admitted to Harvard College with advanced standing.

Like Hrdlička, MacCurdy followed a roundabout path to the study of paleoanthropology. His studies at Harvard were mostly in biology and geology, and he received his bachelor's and master's degrees in 1893 and 1894 respectively. While at Harvard he was encouraged to study anthropology by Frederic Ward Putnam (1839-1915), who was then director of the Peabody Museum, but he did not change his career aims in that direction until 1896. It was also in these years at Harvard that MacCurdy established a personal relationship that was to do a great deal to determine his future course. He was able to get to know Professor and Mrs. Edward E. Salisbury; Salisbury taught at Yale, and Mrs. Salisbury was related to MacCurdy. They were apparently so impressed with his abilities that they offered to finance their kinsman in the European studies that were then deemed so important for young American scientists.[3]

Over the period 1895-1898 MacCurdy pursued a course of study and travel in Europe. At first he undertook biological studies in Vienna, but in 1896 he was among those attending the International Zoological Congress in Leyden when Eugene DuBois first exhibited his "Pithecanthropus erectus" fossils. Theodore McCown called this a "decisive" experience for MacCurdy; another biographer, Hugh Hencken added that MacCurdy's encounter with Java man "so fired his imagination" that it caused him to shift his scholarly focus to the study of human prehistory. Upon his return to the United States in 1898 MacCurdy began his long formal association with Yale University in the fields of anthropology and archeology.

He became an instructor there while he studied for his Ph.D. and acted as curator of the anthropological collections at the Peabody Museum. After receiving his Ph.D. in 1905, MacCurdy joined the permanent faculty at Yale and remained a professor of archeology there until his retirement in 1931.[4]

Though, as has been pointed out, Paleolithic archeology became the principal research interest in MacCurdy's academic career, in the years between 1905 and 1920 he made significant contributions to the study of the archeology and physical anthropology of the Americas as well. He also did organizational work of value to the anthropological profession generally, serving as Secretary of the American Anthropological Association from 1903 to 1916, and as its President in 1930. In addition, MacCurdy helped organize the important 1937 Conference on Early Man held at the Philadelphia Academy of the Natural Sciences which publicized important fossil finds of that era, such as Robert Broom's first australopithecine specimens. Without underestimating the value of this work, MacCurdy's biographers agree that that it is dwarfed by the magnitude of his service to the study of Paleolithic archeology in the United States, and that he did more than any other American of his generation to stimulate interest and develop expertise in this field. His was able to accomplish this not only because of his numerous writings on the subject and his long tenure as a teacher at Yale, but also through his leadership of the American School of Prehistoric Research.

The last named institution deserves special mention because of the importance that was attached to its work by MacCurdy's biographers, who were younger colleagues and thus in a good position to estimate its impact. Founded in the early 1920s the American School's original center of study was Paleolithic sites in France, and its principal mode of operation an 8 to 10 week summer "dig" that gave American graduate (and some undergraduate) students "hands on" experience in archeological methods. In the late 1920s and through the 1930s, however, the school was able to carry on a wider range of activities, principally by engaging in longer term cooperative excavations with European archeologists. The most significant of these ventures was the joint expedition with the British School of Archeology in Jerusalem that yielded the Mount Carmel population of "Neanderthaloid" skeletons, of which more will be said later.[5]

MacCurdy's guidance of the American School was marked by his ability to attract gifted students, his careful cultivation of interest in its activities among prominent Americans, and the unstinting gift of time and energy by himself and his wife, who shared fully in the work of what the MacCurdys came to call "our school." His many contacts with European paleoanthropologists brought in a continual supply of guest lecturers, and much prized access to European museum collections. By kindling enthusiasm for the subject in those who would later go on to become professional archeologists and anthropologists, MacCurdy contributed in a significant way to the growth of the discipline of prehistoric archeology and its spread through the college curricula of America.[6]

MacCurdy appears to have impressed people by the quality of his personality as much as by the quality of his work. His biographers concur in portraying him as an exceptionally kindly, modest and tolerant individual. Hugh Hencken, his successor as head of the American School, remembered him as "so truly kind it actually pained him to believe ill of others," and as a self-effacing man in whose life "scheming and self-advertisement had no part." Earnest A. Hooton praised him as someone who "neither desired nor sought scientific personal distinction and academic preferment," and yet "achieved among all who knew him well a reputation for thorough scholarship and skilled teaching that could be envied by any class-room lecturer on anthropological subjects." MacCurdy was also distinguished by his ability to communicate his enthusiasm for his field of interest and his apparently unusual capacity to make lasting friendships with a great number of the prehistorians and physical anthropologists of his day, particularly in Europe.[7]

These personal characteristics make a very good fit with the style of scholarship that is revealed in MacCurdy's writings. He was a patient accumulator of knowledge, encyclopedic in his thoroughness, who disseminated what he learned lucidly and with admirable organization. Unlike an Osborn or a Hrdlicka, he rarely got involved in controversies, but instead cultivated a detached, even-handed tone; with the partial exception of the "eolith" question, he kept pretty close to viewpoints that represented a careful balance among the best available authorities. MacCurdy's modesty and uncontentiousness go a long way toward explaining his intellectual style; also important, I believe, is the fact that he came to the study of prehistory relatively late in his development. Youthful critical energy and an early mastery of a discipline can often do a great deal to generate fresh approaches to its main problems. MacCurdy, however, was in his thirties when he first started to devote his full energies to prehistoric archeology, a field that even in 1900 possessed a body of basic data of formidable dimensions.

However one attempts to account for the fact, MacCurdy was a synthesizer rather than an innovator, and a synthesizer whose intellectual message maintained great stability over the years. Except for the transition from a unilinear to a multilinear view of later hominid evolution that he shared with most of his American colleagues, once MacCurdy chose what he thought was the most sensible position on the major paleoanthropological issues of the day, he stuck to those positions with little deviation. This allows his work to be considered pretty much as a whole, and close analysis of his views from a chronological standpoint would not enhance our understanding of them. Thus, what follows is organized around key themes in MacCurdy's writings; though some are not fully developed in his early work, all are present within ten years of his first major article on prehistoric archeology, and remain as "leitmotifs" in MacCurdy's later work.

MacCurdy's Conception of Stages in the Evolution of Culture

The first important "theme" in MacCurdy's work concerns the various attempts he made to discern major divisions within the evolution of Stone Age culture. While from the beginning to the end of his career MacCurdy was an adherent of the system of unilinear cultural "epochs" (Chellean, Acheulean, etc.) that he had derived from the French prehistorians of the late 19th century, one can identify quite early in his writings a tendency to superimpose a set of broader distinctions, or "stages," on the traditional list. Though at various times he focused discussion on one "stage" or another, he consistently conceived of the span from the dawn of human culture to the beginning of the Neolithic as divided into three stages.

Carving up the Stone Age into various triads has been a common practice since the 19th century.[8] It is still popular to divide the Paleolithic into a Lower, Middle and Upper. In MacCurdy's time many writers divided the entire Stone Age as he did into an "Eolithic," "Paleolithic" and "Neolithic." The significant things, however, about his division of the period prior to the Neolithic were first, the great emphasis he gave to the notion of an Eolithic stage, that is, a stage of culture where clearly defined tool types were not yet present, and second, the way he tried to heighten the distinction between the Middle and Upper Paleolithic. Indeed this distinction was drawn so emphatically in MacCurdy's discussions of cultural evolution that the Upper Paleolithic came to appear as important as the entire span of the Paleolithic preceding it. Finally, what made MacCurdy's characterization of these issues unique among American paleoanthropologists was the way in which he used the tool types characteristic of his three stages as indicators of supposed ascending levels of efficiency in human culture over time.

The outlines of the three-stage idea were apparent in MacCurdy's pre-World War I writings, though he made no attempt to develop it rigorously. Thus, in an important 1905 article on the "Eolithic" problem, he asserted that "Eolithic industries" were the products of a "low plane of mentality reflecting practically no industrial development," contrasting them sharply with the cultures of the Paleolithic, which he said, had been "signalized by a gradual evolution both mechanical and mental."[9] In comparison with the races of the third, Upper Paleolithic stage however, the folk of the earlier Paleolithic also seemed to him to possess a small capacity for culture. "Progress was slow" during the Chellean and Acheulian epochs, MacCurdy asserted in 1913, and the last race characteristic of the second stage, the Neanderthals, had been "a race of coarse mental and physical fiber" as well. The people of the Upper Paleolithic, however, he believed to be as much advanced morphologically as they were culturally over their predecessors, since they represented a Homo sapiens type "more nearly akin to the modern Europeans than to the archaic Mousterians." And the great gap between the Mousterian and Upper Paleolithic populations was not just a matter of gross morphology; since culture depended "largely on the inventive faculty and the faculties for transmitting racial experience [i.e. language]" inborn intellectual differences had to be involved as well.[10]

Though to modern students MacCurdy's first evolutionary stage would seem fanciful and the distinction between the second and third overdrawn, one can easily point out the reasons he believed his scheme accurate. First, in regard to the "Eolithic", if one were inclined to accept the existence of this "rude industry antedating the Paleolithic" (MacCurdy's reasons will be discussed later), one had to be prepared to accept a long and essentially static course for it as well. Stones answering to the description of eoliths had been found in deposits at least as old as the Miocene and continued into Pleistocene strata showing little, if any, change of form; the many Eolithic "epochs" and industries that had been identified by European workers, were, as MacCurdy pointed out, "based on stratigraphy and not on industrial characters."[11] On the morphological side of the equation the only remains that were generally accepted as pre-Pleistocene (prior to the attempted redating of Piltdown in the 1920s) were those of Dubois' "Pithecanthropus." Since there was no stone tool industry found with Java man, one was free to associate his primitive morphology with the primitive "Eolithic" stage of culture. Even if one accepted the revised, early Pleistocene date for Pithecanthropus that became standard by 1920 no modification in theory was necessary; all one would have to argue was that the survival of this Pliocene-like form unchanged into the Pleistocene showed that "Eolithic" races were as static in their bodily form as their culture showed them to be psychologically.[12]

MacCurdy's assertion that the earlier part of the Paleolithic displayed a rate of physical and cultural evolution faster than the "Eolithic" could also be grounded on the work of excellent contemporary authorities. As we have noted in our discussion of Osborn, the first decade of this century saw the attempt by Albrecht Penck to extend the earlier industries of the Paleolithic back in time.[13] In a series of Silliman lectures at Yale in 1908 Penck had assigned the Chellean industry a Second Interglacial date and argued that Mousterian man had been extant as far back as the Third

Interglacial or even the Third Glacial epoch. The decade had also witnessed the discovery of "Homo heidelbergensis," a fossil jaw "at least as old as the early Quaternary,"[14] that nearly all students accepted as generically similar to modern humans. Putting these two facts together, MacCurdy felt quite safe arguing that "the Chellean industry already represents a degree of intelligence that must stamp its author as distinctly _Homo_;"[15] if the evolution of culture in the early and middle Paleolithic thus coincided with the evolution of early _Homo_, this would explain the more rapid progress these cultures had made in contrast to eolithic industries, which had been produced by individuals of more "primitive" genera.[16] In addition, lengthening the time span of the earlier Paleolithic cultures allowed the inference that their rate of progress had been slow in comparison to the Upper Paleolithic, which increased the likelihood that they had been produced by intellectually inferior hominids.

The trend of research in the early 20th century regarding the fossils of the Middle Paleolithic also squared well with MacCurdy's three-stage idea. As we have seen the period around 1910 was the time when Marcellin Boule "proved" the Neanderthals ape-like in so many characters that they could not have evolved into _Homo_ *sapiens* in the time alloted for such a transition. As early as 1910 MacCurdy showed himself a convert to Boule's views on the issue most important for the question of mental and cultural evolution -- the inferiority of the Neanderthal brain. He accepted the marks of inferiority *vis à vis* modern man -- the supposedly simple frontal convolutions, the relatively small frontal lobe, and so on -- described by Boule and Raoul Anthony on the endocasts of the La Chapelle-aux-Saints and La Quina Neanderthals. Characters like these, and the more flattened general outline of Neanderthal brains, MacCurdy argued, outweighed the relatively large cranial capacity of Neanderthal skulls. Quality took precedence over mere quantity, and revealed the Neanderthals as the intellectual inferiors of the Upper Paleolithic races that succeeded them.[17]

Though the idea of three stages in prehistoric man's mental progress emerged earlier in MacCurdy's career, its most coherent expression did not appear until the 1920s. In his *magnum opus* of 1924, *Human Origins,* he outlined a theory of the technical evolution of stone tools which not only kept the three stage idea intact but also provided a functional interpretation of the tool types characteristic of each stage. The theory rested on the notion that all of the tool types associated with Stone Age populations could be grouped within three basic categories. A "primary" tool was one which could be put to use immediately in the form provided by nature, such as a hammerstone or a naturally sharpened "eolithic" flint flake. A "secondary" tool, however, required human modification -- either through intitial shaping or retouching -- before it could serve human purposes; most of the recognizable stone tools of the Paleolithic ("hand-axes," "scrapers," etc.) could be considered "secondary." Finally, a "tertiary" tool MacCurdy defined as one which required the use of primary and secondary tools in its manufacture, but was itself used for a purpose other than the making of implements. The examples he gave were tools of materials other than stone -- such as bone harpoons, sewing needles, and so-called "dart-throwers" made of bone or antler.[18]

If one surveyed the distribution of the three tool categories across the Eolithic and Paleolithic eras, MacCurdy asserted, one could make out the three distinct stages of cultural evolution clearly. In the Eolithic nearly all tools were primary, with the exception of a few extremely crude secondary tools, mostly flakes of flint. In addition, no significant progress in tool-making technique occurred over the entire Eolithic era.[19] The Eolithic was followed, however, by a phase of evolution in which secondary tools increased in sophistication and multiplied in type. This phase supposedly lasted through both the Lower and Middle Paleolithic. Yet while progress took place -- for example in fineness of flint chipping during the Chellean and Acheulian, and in the preparation of flint cores prior to the removal of flakes during the Mousterian -- MacCurdy argued that no major improvement in basic methods had occurred.[20]

A basic improvement was supposedly made during the final phase -- the Upper Paleolithic, when a breakthrough had occurred which had "transformed" human culture. Numerous additions to the stock of specialized secondary tools had become possible through the use of "blade-like" flake tools struck from prepared flint nuclei. These new secondary tools ("gravers," "burins," etc.) had for the first time allowed the creation of an array of tertiary tools, and had paved the way for the achievements in the fine and decorative arts for which Upper Paleolithic peoples had become justly famous.[21]

MacCurdy's revised scheme was interesting, since it represented an abstract way of distinguishing Upper Paleolithic populations from their predecessors -- abstract in that it tried to go beyond a litany of specific traits possessed by Upper Paleolithic cultures but supposedly absent earlier. Instead, he was invoking a set of general categories that measured cultural complexity. Since only Upper Paleolithic peoples had reached a "tertiary" level, one could infer their mental superiority directly. That is, by establishing the above trichotomy one could argue that Upper Paleolithic flint blades, bone tools and art were not improvements in a continuous chain of invention stretching across the entire Paleolithic, but rather indicated a saltation which would agree with the belief in an abrupt transition of "races" between the Middle and Upper Paleolithic.

While his triadic formula was interesting, MacCurdy did not fully succeed in establishing the qualitative distinction that he was aiming at. When one examines, for example, the Mousterian "secondary" tools described in Human Origins, one encounters types such as "spokeshaves" and "drills."[22] One needs only to theorize that the typological description of these tools is an approximation of their function to infer that such tools or similar ones could have been used in fashioning other implements from perishable materals like wood. A wooden club or spear so fashioned would fall exactly within the definition of a "tertiary" tool as one which "requires the use of primary and secondary tools, and whose ultimate use is not in the shaping of implements." Thus, one would be reduced again to less discrete and more continuous kinds of distinctions

-- e.g. that the "tertiary" tools of the Upper Paleolithic were more finely crafted and more varied than the Mousterian, because of a similar degree of superiority in the secondary tool kit. The cultural chasm that he described was thus more like a valley.

MacCurdy and the Neanderthal Replacement Theory

That MacCurdy would ignore this problem in his account of cultural evolution was not really surprising, for a rigid separation between the "races" and cultures of the Middle and Upper Paleolithic had become an article of faith for him by 1924, and a major theme in his work. We have already noted his conversion to a belief in the mental inferiority of Neanderthal man by Boule. Along with this conversion MacCurdy had also come to accept the theory that Neanderthal man represented the end point of an extinct side branch of the human family.

This change from a unilinear to a multilinear conception of human evolution had apparently come a bit before Osborn's shift in the same direction. In 1909, while describing the Mauer jaw MacCurdy listed the various Neanderthaloid lower jaws that had been discovered, and asserted that the latter jaws "represent one and the same stage in the evolution of Homo sapiens. That this stage is intermediate between recent man and Homo heidelbergensis, a careful comparison of the specimens in question furnishes ample proof."[23] A year later, however, under the impact of Boule's interpretation of La Chapelle and the recent description of the "Aurignacian" skeleton found at Combe Capelle, MacCurdy had changed his mind; the Neanderthals could not have evolved into the "higher types" of the Upper Paleolithic in the time available for the transition. The close juxtaposition of these populatons in time and space was thus "a fact difficult to explain without recourse to the theory of an influx of new blood."[24]

Once converted to the view that "Homo neanderthalensis" and early Homo sapiens represented different lines of late Pleistocene hominid evolution, MacCurdy proved to be a strong promoter of the idea for the rest of his career. Indeed, in his continuing discussions of the field of human evolution generally, the issue of the Neanderthal - Upper Paleolithic relationship captured his attention more than any other single evolutionary problem. This was especially true of Human Origins. As the latter was a synthetic work, a summary of extant knowledge in paleoanthropology, the volume of evidence on a given topic had to influence the amount of space devoted to it; and in 1924 only the Mousterian and later "epochs" had produced large data samples that clearly associated specific tool traditions with specific forms of fossil hominids. The only older fossil remains that were generally accepted were Piltdown, which as we have seen was found with a few crudely chipped flints of indeterminate status; the Mauer jaw, which possessed no associated cultural remains; and Java man, for which the latter was also true. Similarly, the pre-Mousterian tool traditions -- the Chellean and Acheulian "hand-ax" cultures, had no associated fossil remains until after 1930.[25] By contrast Western and Central Europe had yielded up numerous finds of hominid fossils in Middle and Upper Paleolithic contexts. And while early Homo sapiens fossils had been uncovered in deposits bearing tools from the major Upper Paleolithic industries -- Aurignacian, Solutrean, and Magdalenian, "Neanderthal-type" fossils had all come from so-called "cold" Mousterian contexts, with the possible exception of the Ehringsdorf (Taubach) jaw.[26]

For MacCurdy, the general conclusion to be drawn from this evidence was unavoidable. "Neanderthal man is the equivalent of Mousterian man," he asserted;[27] furthermore both cultural and morphological evidence showed the Neanderthals to be inferior to the Upper

Paleolithic populations that followed, so inferior that even "the monogenists must admit" the two groups to be of different species.[28] The appearance of the new "race," "of a type more nearly akin to modern man," which superseded the Neanderthals was also "coincident with a marked change in the character of the cultural remains and especially with the origin of art."[29] The record revealed a physical "gap" between the "Mousterian type and the Aurignacian type" that had not yet been "definitely bridged" by fossils of intermediate morphology.[30] And as far as culture was concerned, MacCurdy contended, "the race that left the archaic Mousterian industry was either careless or incapable of producing anything but indifferent results in the way of chipping flint,"[31] while the Upper Paleolithic race, as shown by their sophisticated tools and their art were "men of new ideas, practical as well as aesthetic."[32]

Though the language stopped short of Osborn's paean to his "Paleolithic Greeks," MacCurdy's statements on the Neanderthal question strongly supported the same broad conclusions -- that the Neanderthals and their successors had evolved separately, and that the succession itself was an instance of the abrupt replacement of an inferior race by a superior one. Yet interestingly, just as in Men of the Old Stone Age and perhaps to a greater degree, many of the details MacCurdy reported in Human Origins undermined the simple scenario summarized above. Thus, in discussing the fossils of the Upper Paleolithic, he noted that the Combe Capelle, Predmost and Chancelade skeletons all appeared to contain characters reminiscent of those in Neanderthal man, if not duplicating them.[33] Chancelade, he even conjectured, might represent the product of racial mixture between "Homo neanderthalensis" and Homo sapiens.[34]

In his analysis of cultural detail MacCurdy pointed out traits in which the distinction between the Mousterian and Aurignacian was not complete. Thus the rudiments of a bone tool industry appeared to exist in the Mousterian deposits at LaQuina in France, and at the caves of Wildkirchli and Drachenloch in Switzerland.[35] Also, the beginnings of a blade tool industry had been found at some late Mousterian sites like Le Moustier and Audi in France, an industry which seemed to show a smooth transition to the Aurignacian blade types which existed in the layers above them.[36]

Finally, there was evidence of continuity in MacCurdy's discussion of cultural practices as inferred from the available archeological data. Of particular interest was his chapter on the "Stone Age Culture Complex," in which he tried to trace the origin and development of several key aspects of human culture. In the critical area of religion he painted a picture of gradual transformation from Mousterian to Aurignacian times. "Mousterian man," he wrote,[37]

> whose ideas of art were so primitive as to escape detection, took pains to bury his dead. He evidently believed in the hereafter; one however that was material, since food was buried with the departed, presumably to meet material needs. In a hereafter like the present life there would be need of tools and weapons; these were also buried with the dead.
>
> The Aurignacian and later races developed the burial rite further. They had other ways also of leaving imperishable records of religious practices, chief among them being art. Religion is older than art and may have served as the fertile soil in which art first took root, but as a means of tangible and imperishable religious expression, art justifiably claims first place.

Similarly, in regard to the practice of hunting, MacCurdy treated the Paleolithic as a continuum, with the hunters of the Later Paleolithic adding only the dart thrower and the harpoon to the basic tool kit of wooden club, bola, flint knife and javelin known to the Mousterians. This continuity was highly important, for MacCurdy saw hunting as an essential human adaptation. Hunting was, he aserted, "as old as humanity; when the human precursor traded the arboreal for the terrestrial domain, he became a hunter by necessity as well as by choice."[38]

Taken together, these statements about cultural issues present an image of inconsistency in MacCurdy's treatment of the Neanderthal problem -- while arguing replacement as a general conclusion, he was providing some important details that were consistent with continuous evolution. There was also inconsistency in his discussion of one key source of the morphological evidence against the Neanderthals -- i.e. the interpretation of endocranial casts. By 1924, James Symington's strictures on the reliability of endocranial casts were apparently well known. Symington, a British neuroanatomist, had made some careful comparisons of endocranial casts and brains in modern humans. His inability to predict the convolutional pattern of the brain from the form of the so-called "brain cast" had led him to question the usefulness of these casts in reconstructing the mental abilities of fossil hominids. MacCurdy himself seemed to accept Symington's warnings, at one point saying that Symington had "presented ... a series of casts <u>proving</u> that the endocranial cast does not give definite information regarding the features of the brain in detail [my emphasis]."[39] Nevertheless MacCurdy substantially repeated the conclusions of Elliot Smith on the Piltdown brain and those of Boule and Anthony on the Neanderthal brain, without indicating that Symington's critique was directed particularly at these two efforts at reconstruction of fossil brain form.[40]

The case of the Neanderthal brain was especially revealing. Citing Boule and Anthony, MacCurdy took special note of the supposed fact that in Neanderthal man the surface area of the frontal lobe was only about 36 percent of the total area of the corresponding cerebral hemisphere. In modern humans this ratio was said to be 46 percent and in great apes 32 percent. In regard to the development of the frontal lobe, therefore, "Homo neanderthalensis" allegedly stood "closer to the anthropoids than to _Homo sapiens._"[41] What MacCurdy did not mention was that fixing the relative proportion of the frontal lobe depends on an accurate delineation of the course of the Sylvian fissure and central sulcus of the brain; it had been one of Symington's principal arguments that _neither_ of these landmarks could be fixed with accuracy on endocranial casts. Similarly MacCurdy reported a certain resemblance to "pithecoid" brains in the Neanderthal third frontal convolution, an area important in human speech production, a conclusion which would also rely on accurate reproduction of the region around the Sylvian fissure. The same uncertainty would also have applied to his claim that the Neanderthal lunate sulcus, an important landmark in the occipital region, was also apelike in form.[42] Thus, without a concomitant attempt to provide some criticism of Symington's arguments the endocranial evidence did not really add up to MacCurdy's general conclusion that "as an organ of cerebration the brain of Homo neanderthalensis was evidently much inferior to the brain of any living group of Homo sapiens, however lowly."[43]

Multilinearity and Piltdown

Given these problems, why did MacCurdy support the two species theory and replacement theory so unquestioningly? One of the reasons had to be his willingness to fit in with the general trend of the times among British and French paleoanthropologists. Not only were Keith, Boule, Osborn et al. on the same side of the Neanderthal issue; also, the major writers tended to favor multiple lines of descent in their general theories of human evolution as well. MacCurdy proved to be no exception to this general drift, though his own movement was held back a bit by the sceptical stance he took toward some of his colleagues' claims.

As with the Neanderthal question, the process of conversion toward polyphyletism began between 1910 and 1915. At first he was merely reporting the polyphyletic ideas of others, and showing some resistance to them. The extreme theory of the German Hermann Klaatsch (1863-1916), who believed that one major line of fossil hominids had evolved from a chimpanzee-like ancestor while another line had an orang-like parentage, was obviously too much for him; the comparative anatomical evidence of Keith for man's special closeness to the African pongid stock seemed too strong.[44]

In 1909, a theory that MacCurdy attributed to the Belgian archeologist Rutot, one which hypothesized the existence of a so far undiscovered "progressive" line of hominids ancestral to man evolving in parallel with the more ape-like line leading from "Pithecanthropus" to "Homo neanderthalensis," appeared less objectionable. Still, MacCurdy hesitated because he felt unwilling to relegate "Homo heidelbergensis" to Rutot's proposed side branch; for the time being the latter fossil should, he thought, be allowed to retain "a fundamental position in the line of human evolution."[45] A few years later he accorded similar respect to Keith's belief that _Homo sapiens_ had co-existed during the Pleistocene with a more primitive hominid line that culminated with the Neanderthals, but remained sceptical of the antiquity of the two English skeletons which Keith took for representatives of mid-Pleistocene man.[46]

The fossil that put MacCurdy firmly in the camp of those who accepted multiple hominid lines in the earlier Pleistocene was Piltdown man. If one accepted the Piltdown braincase as genuine and geologically ancient there seemed to be little other choice, as it contained a relatively high forehead and smooth brow ridges dramatically different from those of both "Pithecanthropus" and the Neanderthals. That he was predisposed to accept Piltdown he made clear in his first discussion of the discovery; he had long believed, he asserted, in "the prehistoric possibilities of southern England because of the outcrops of flint-bearing chalk ... Of all raw materials flint is perhaps the best suited to tempt nascent Homo to become a tool user."[47]

Though MacCurdy readily granted Piltdown an important place in the human family his conception of where it fit in was vague. He seemed to want it inserted in a way that would least disturb the positions of existing fossils. Thus, unlike the later Osborn he refused to take Piltdown as a "progressive" ancestor or close relative of Pleistocene Homo sapiens; rather, he adhered to Elliot Smith's portrait of the Piltdown endocranial cast as exceedingly "primitive," and thus did not see early Pleistocene hominids as composed of several groups evolving at different rates. The three fossils from that era -- Piltdown, "Pithecanthropus" and "Homo heidelbergensis" -- merely represented, he said, "sections, not of one branch but of different branches of the same family tree."[48] Anticipating Hooton's portrayal of "multilinearity" in hominid evolution, MacCurdy argued that the coexistence of these forms illustrated the "fact" that "in the Lower Quaternary the differentiation among the Hominidae had already progressed much farther than has generally been supposed."[49] What the evolutionary significance of this "differentiation" was he did not speculate about.

Even if the braincase of Piltdown man could be easily worked into one's evolutionary scheme, the fossil still presented a major problem of interpretation in the association of the braincase with the jaw. Endocranial "primitiveness" aside, the braincase was large, and as modern in form as Upper Paleolithic specimens while the jaw appeared considerably more ape-like in form than the Mauer jaw. At first MacCurdy got over the problem by invoking the authority of Elliot Smith: as man's massive and complex brain was the key feature that separated him from the other primates, one would expect it to have led the way in his evolution. Major expansion in brain size and complexity over anthropoid conditions would have been both a precursor of and precondition for "the refinement of the features and the somatic characters in general."[50] This fact, and the unlikelihood that two hitherto undiscovered primates would have come to light at the same time, were enough to still his doubts about attributing both skull and jaw to a single creature, Smith Woodward's "Eoanthropus."[51]

The initial mood of easy acceptance was not to hold, however; potent criticisms of the Piltdown jaw had surfaced. Boule had grumbled that it was undoubtedly an ape's mandible, probably a chimpanzee's. In the U.S. Gerrit Miller had compared casts of the Piltdown jaw to a series of anthropoid jaws, and had identified several characters that it shared with chimpanzees but with _no_ hominid jaw, living or fossil.[52] By 1916 MacCurdy had come around to their side. Geological association, he asserted, could "never be made to take the place of articulation; and so far as Piltdown is concerned, nothing short of the actual articulation of the mandible with the skull would have sufficed to outweigh the lack of harmony existing between those parts."[53] Thus, he argued, the "tenable" position regarding Piltdown would be that two creatures were represented: a hominid, "Homo dawsoni," and a fossil chimpanzee. "As for the man of Piltdown," MacCurdy concluded, "he still exists and is quite as ancient as he was before the revision, which is saying a good deal; even if he is robbed of a muzzle that ill became him."[54]

This air of mild and breezy scepticism was not destined to last either. MacCurdy's objections dissipated under the weight of the British counterattack against Miller and Boule. The discovery of the so-called "Piltdown II" fragments in 1915 -- which included both pieces of skull bone and a lower molar presumably from a second "Eoanthropus" -- eventually took its toll; it seemed to MacCurdy, as to Osborn, improbable that such a coincidence could happen twice.[55] X-ray analysis of the Piltdown teeth provided evidence of allegedly hominid affinities that he was also willing to accept.[56] MacCurdy himself was able to examine the original specimens in 1922; as it had done for Osborn, personal examination helped convince him that the skull and jaw somehow belonged to each other, and made him more sympathetic to the views of Piltdown man's English custodians.[57]

To be sure, he still had some lingering objections. The isolated canine tooth that had been found at Piltdown by Teilhard de Chardin (1881-1955) was still too ape-like for his taste, even if one accepted the rest of "Eoanthropus."[58] Then on the mandible itself the inner margin of the bone possessed a marked "simian shelf," an anthropoid character that contrasted with the hominid pattern of the molars. "In order to surmount such an anatomical obstacle," he said, "one must invoke a wider range of individual variation within the genus Homo (Eoanthropus included) than has hitherto been considered ample."[59] But invoke it he did. In Human Origins, "Eoanthropus" -- jaw and all -- received equal billing with the other forms of early Pleistocene hominid, and MacCurdy derived the same vague lesson from the fossil that he had derived in 1913 -- namely, that it showed how far the various forms of Homo had "differentiated" from each other. Thus, Piltdown indicated that "we shall have to go a long way back in the past to find the parting of the ways between the ancestor of man and his nearest of kin among the apes."[60]

MacCurdy's Failure to Explain Multilinear Evolution

Though MacCurdy was willing to accept the evidence for multiple lines of hominid evolution in Human Origins, he was still apparently reticent about drawing those lines with any exactness. In a long chapter on fossil hominids he included no "family tree" sketching hominid phylogenetic relationships. He saw all the early Pleistocene fossils as closely related to humankind:

"Pithecanthropus'" kinship with _Homo_ was called "very close," Piltdown man represented " _Homo_ both physically and culturally," the Mauer jaw "physically measures up to that required for _Homo_. "[61] In discussing Java man, he described three major theories of that creature's phylogenetic status but failed to indicate which alternative he favored.[62]

Interestingly, while MacCurdy supplied no explanation for, or even an explicit decription of, the various hominid phyla that were allegedly in existence during the Pleistocene, he was quite forthright in expressing his views on the early phase of human evolution that encompassed the emergence of hominids from the anthropoid stock. On this issue he was quite clearly on the side of the "ape-man" theory. Following the lead of Elliot Smith, MacCurdy held that arboreal life had developed in the hominoid ancestors of man a complex of adaptations that had interacted with each other to make ground dwelling and tool-use possible -- a complex consisting of "the hand, a brain that is fairly well balanced on the spinal column, and stereoscopic vision."[63] He also agreed with the theory of Keith, as developed by William K. Gregory, that the physical characters forming the basis of the human erect posture showed so many homologies with the apes that _Homo sapiens_ must have had a reasonably large-bodied hominoid as an ancestor.[64]

Again like Elliot Smith, MacCurdy thought that the transition from ape to man was somehow provoked by the expansion of the brain.[65] How this had occurred did not seem entirely clear from his discussion, but when it occurred was clearly stated, and it was much later than "ape-man" opponents like Osborn could accept: the record, though "fragmentary," pointed to "a conjunction of the physical and cultural requirements necessary to constitute nascent *Homo* somewhere in the late Tertiary epoch."[66]

Of course, "How?" is an important question, perhaps more important than "When?" for the purpose of understanding a writer's conception of humankind's place in the evolutionary scheme. That MacCurdy was somewhat vague in describing the process of hominid emergence was in itself significant, but there were also hints in the language he used that he saw the process as being directed from within. What one might call this "inner-directed" quality could have been the result of a belief in orthogenesis, but MacCurdy never spelled out his evolutionary principles to this degree in his writings. Instead, he merely stated his view on the specific case of humankind: at some time during the later Tertiary the anthropoid ancestors of man reached a critical point of transition, when the complex brain that had evolved during the arboreal phase made a momentous choice possible. Arboreal life appeared to be something of a dead end for large-bodied creatures, as both man's ancestors and his great ape cousins were becoming.[67] For the proto-hominid however, "there came a time when the call of the brain for the freedom of the hand outweighed the needs of the hand for support; and in winning the freedom of the hand, the brain won its own freedom to a field of almost unlimited possibilities for expansion, for hand freedom means erect posture and a brain case posed where it may best expand."[68]

Thus, while ground-dwelling and tool use would both have provided stimulus for the further development of the brain, it was the large brain itself that had first made the <u>choice</u> that set the hominids' career on the ground in motion. As MacCurdy described it,[69] for man's arboreal ancestors,

> there remained little else to gain and much to lose; so by degrees arboreal life was abandoned ... It was a momentous step, the first step toward man's conquest of his environment and hence of the earth. Had he been content with tree life, he would never have built up that fabric which is the result of cultural evolution.

MacCurdy was neither unique nor original in his espousal of a brain-directed course of hominid development. As has been noted already, Elliot Smith had pioneered this theory and had done the most to spread it.[70] More important than the search for the sources of this idea is an analysis of the intellectual problems it raised and of possible reasons for its attractiveness. A principal difficulty, and one that was even more significant for the work of E.A. Hooton than for that of MacCurdy, was that the theory did not explain the required divergence in the paths of the various Tertiary hominoids including what one might call the "proto-hominids."[71] Specifically, what pre-adaptation had given the latter creatures the "supra -anthropoid" brain that had established them in their evolutionary career?

More important for a writer like MacCurdy, whose main professional concern was human evolution during the Pleistocene, was that the model presented hominids as a group that evolved by exploiting an opportunity rather than by responding to environmental pressures. There was nothing inherently wrong in portraying hominids as a generalized, opportunistic group, but such a conception made it difficult to explain the alleged fact that among later hominids there had appeared a number of phyla possessing different levels of mental complexity. In short, what would have caused the Neanderthaloids to turn out so differently from their predecessors?

On this important issue, a comparison with Osborn is instructive. Despite his inconsistencies and oversimplifications in detail (faults which MacCurdy was better at avoiding), Osborn was aware enough in his later writings of general principles of evolution to search out unifying theories that would explain the multilinear course of hominid evolution. The core ideas, of course, were the central Asia theory and the forest-upland plateau dichotomy that was integral to it. The difficulties raised by these ideas have already been emphasized.[72] Still, in contrast to MacCurdy, Osborn did realize that a theory which implied different rates of bio-cultural evolution in the several branches of the hominid family tree required some sort of adaptational explanation. Without a felt need to meet such a requirement MacCurdy's formidable knowledge of the details about Paleolithic culture and Pleistocene hominids was never put to maximum use.[73]

That MacCurdy, and later Hooton, undervalued the roles of environment and adaptation in what has to be marked as one of the critical points in hominid phylogeny can be related, if speculatively, to their personal backgrounds. Though nearly a generation apart in age, both men had reached young adulthood in small towns in the American heartland; both had achieved success in the narrow field opened to them. But the future prospect of professional status or some other form of respectability in their "small pond" had not been enough for either. As young men, both MacCurdy and Hooton had made the decision to leave behind an apparently comfortable but potentially stultifying environment and moved into the wider, more stimulating as well as more risked-filled world of a metropolitan university. Further, both had chosen the exercise of the intellect as their path to a more fulfilling life. For them personal choice,, taking a risk, and the use of their native wits had paid off. Perhaps it had been the same for their remote ancestors. This "tried and true" theme of American middle class culture might easily have filtered into their conception of the human past.

While MacCurdy's general attitude toward the emergence of the hominids may have helped to prevent him from dealing with important evolutionary issues regarding Stone Age peoples, there were other themes or habits of thought involved as well. One, which we have also encountered in relation to Osborn, was the habit of resorting to race as an explanatory tool. With Osborn, however, race was a major theoretical construct, to which he gave considerable attention and which he tried to integrate into his general biological theories. For MacCurdy, race led a more modest and a more independent existence -- it merely provided a convenient way of organizing certain facts, a way that helped him avoid considering their theoretical implications rather than confront them.

A minor, but revealing instance of the way MacCurdy used the idea of race occurred in one of his discussions of Paleolithic art. In describing the way in which artists portrayed the female figure, he contrasted two statuettes -- one of the common and voluptuous "Venus" variety and another depicting a more slender, graceful figure; the difference, he asserted, probably resulted from the fact that the model for the latter had been a representative of "a slender, probably superior race," even though both statuettes had been found in the same archeological context in the same deposit. A potentially puzzling juxtaposition of contrasting artistic styles could thus be resolved with minimum effort.[74]

Much more important than this glimpse into a late Victorian aesthetic sensibility was the way MacCurdy used race to help explain the Neanderthal problem. We have seen that he looked upon the Neanderthals as a homogeneous racial "type" which inhabited Europe during the Mousterian "epoch." In Human Origins and other writings, however, he went beyond this, and spoke of a "Mousterian race" which was superseded by an "Aurignacian race". To be sure, MacCurdy was willing to allow "negroid" types such as the so-called "Grimaldi" skeletons into his Aurignacian group;[75] he was thus less stringent than Osborn about racial purity among the Cro-Magnons sensu lato. Still, by using terms like "Mousterian race" at all, he was assuming identity between a cultural group and a distinct physical type -- an identity that fell far short of being demonstrable, given the scarcity at the time of fossil remains and cultural sequences from areas outside Europe.

At least one review of Human Origins pointed out the danger of such a procedure, but when MacCurdy responded to the criticisms of his reviewers in the pages of the American Anthropologist he failed to speak to this particular issue.[76] One might argue that the question was too insignificant to claim his attention, but the possiblity that this was the case is in itself worthy of note. Race most likely seemed like an obvious way to sum up a pattern of correlated physical and mental differences between populations, so obvious that it could be employed presumptively as an explanation for the demise of the Neanderthals. The existence of races on different intellectual and cultural levels, the inevitable conflicts among them, and the replacement of the inferior by the superior groups may have seemed like general truths; the evidence that existed regarding the specific case of the

Neanderthals fit the pattern so well that the burden of proof was on opponents to prove the theory of race replacement unlikely.

If attitudes about race had their influence in forestalling a full discussion of the problems entailed by multiple hominid phyla, there was another, and perhaps more potent factor that came from McCurdy's assumptions about the field of archeology itself. For MacCurdy adhered to an archeological model of human evolution that assumed unilinear progress during the Stone Age. We have already seen that MacCurdy tended to portray the Stone Age as a triadic series of successive "stages" of development. Underlying this conception was a firm commitment to the theory that the various industries of the Stone Age could be arranged as a unilinear series of cultural "epochs" where each epoch possessed its characteristic tool "types" and occupied its own, unvarying place in the geological strata.

The scheme of cultural "epochs" was the received tradition in the early 20th century, a tradition that owed its strength largely to the pioneering work of the French school of prehistorians.[77] But one needs to invoke more than tradition to explain its centrality in MacCurdy's work. It provided the ordering principle behind all his synthetic writings, so that the story of the Stone Age became mainly a narrative of the successive "epochs" -- Chellean, Acheulian, Mousterian, Aurignacian, and so on.[78] In the record of Stone Age culture, MacCurdy believed, "there is everywhere orderly development, marked either by refinement of existing forms or the appearance of new ones. The result is that a given combination of cultural phenomena has its definite stratigraphic position."[79]

Much of the continuing attraction of the unilinear epoch theory for archeologists like MacCurdy must have grown out of the fact that once developed, it provided a clear and meaningful set of problems for further research. It is in the nature of historical generalizations like cultural "epochs" that their boundaries cannot be easily established in time or space. Defining the category securely required attempts to analyze all the particular locations where the culture might be present. Fixing the geological associations of "index" tool types in such deposits could either provide much needed confirmation of datings already established elsewhere or establish new ones. Likewise every sequence of tool bearing strata provided a potentially important test of the hypothesized succession of "epochs." Each new site could also shed important light on the geographic distribution of the peoples of a given "epoch." Finally, differences in the dating of the same culture stage at different sites could help pinpoint centers of emergence or trace the supposed directions of cultural diffusion or the migrations of prehistoric peoples.

As MacCurdy once pointed out in defense of the European classification system, the prehistorians who worked within it did not consider it immune from modification by new facts. Admittedly, it had only been adequately tested for central, southern and western Europe. Nevertheless, he argued,[80]

> a certain definite succession of cultures already holds good over a large area. ... When Asia and Africa have been studied with equal thoroughness there will be much to add and no doubt some to subtract. There can be a system of classification and still allow [sic] for all sorts of local rises and falls of the culture barometer as well as movements of peoples over large areas. ... The wonder is that any system could be discovered, and I say discovered rather than devised advisedly, which could so long withstand so heavy and complex a strain. The system in its elemental outlines still survives; and where there is life there is hope, and the possibility of further growth.

All in all, the idea of cultural epochs provided early 20th century prehistorians with a simplified, but clear example of what Thomas Kuhn calls a "paradigm" -- an interrelated set of theoretical statements that answers the "big" questions that arise during the amorphous, embryonic stages of a science, and then sets the agenda of "puzzles" that practitioners of "normal science," the day to day working out of scientific details, will proceed to solve.[81] Indeed, MacCurdy often took the opportunity to chronicle refinements in the details of the Paleolithic system, refinements which in his view marked the scientific progress of his chosen discipline.[82] For example, MacCurdy's praise for Penck's effort to improve the correlation of Paleolithic industries with glacial events in Europe has already been mentioned.[83]

Even more significant for MacCurdy's work were the revisions and refinements produced by the Abbe Breuil, whose attempts to clarify the sequence and dating of European Paleolithic industries carried immense weight with prehistorians generally.[84] For example, it was Breuil's research on the Aurignacian "epoch" that placed it firmly between the Mousterian and Solutrean, rather than betwen the Solutrean and the Magdalenian, the place it had occupied in de Mortillet's scheme. As early as 1910 MacCurdy was citing Breuil's placement of the Aurignacian and the relative dating he had established between it and the Mousterian as evidence for regarding the two cultures as the products of different and ultimately competing races.[85] As late as 1931 MacCurdy was still singling out the French scientist for special praise, portraying him as the person who had done the most during the 1920s to clarify the sequence of cultural evolution in the Paleolithic.[86]

Not only did MacCurdy prominently feature these clarifications of the "paradigm" in his reportage and review articles, he also made independent confirmation of the received cultural sequences a major focus of his own researches in the Old World. Thus in 1904-5, while researching the "Eolithic" question, he not only used his time in Europe to view collections and discuss theory with the champions of the Eolithic idea, but also made sure to explore some of the sites for himself and to dig up specimens of "eoliths" that matched the types discovered earlier.[87] More organized but similar in motivation was the excavation he undertook in 1914 of La Combe, a Paleolithic cave shelter in France; he wanted to test the sequence of artifacts in the various layers, he said, to see whether it conformed to the typical pattern, which of course it did.[88] Later, when he brought the American School for Prehistoric Studies to France for its first summer of research, its principal task was a similar excavation at the cave of La Quina.[89]

Because of the tremendous amount of attention that he devoted to the problem of cultural sequences, the tables of cultural "epochs" that MacCurdy produced in synthetic works like <u>Human Origins</u> undoubtedly represented more to him than a dry codification of details. Instead the production of an accurate and up to date synopsis of Stone Age culture must have seemed to him an achievement that fulfilled one of the prehistorian's major scientific duties.[90] Some conception of his role as a sort of keeper of the archeological faith may indeed have been an important motivation for him, since the example of European authorities is not really enough to explain his continued adherence to the theory of unilinear sequences. This becomes clear from the fact that MacCurdy persisted in his devotion to it after his main European authority, the Abbe Breuil himself, had begun to question its usefulness.

An examination of two tables from the Coming of Man, a popular work of 1931 updating the major points of Human Origins, reveals this discordance concisely. In discussing the dating of Paleolithic industries MacCurdy used a table which, he said, adopted "the main points in Breuil's synchronism of European glaciations and European cultural epochs, slightly modified." In this table were several instances where Breuil identified the simultaneous existence of typologically distinct industries, a fact which clearly showed that Breuil was no longer thinking in terms of "epochs" at all.[91] Yet when it came time to synopsize the evolution of Paleolithic culture, MacCurdy retained his unilinear series of "culture epochs" by merely dropping those industries that were not included in the traditional scheme, i.e. Breuil's "Clactonian," "Micoquean," and "Levalloisian."[92]

Neither ignorance nor carelessness, of course, had caused this condensation of the cultural record; as Human Origins revealed, MacCurdy had an encyclopedic familiarity with the research on Paleolithic Europe. Instead, he merely seemed unwilling to confront the complexities that would be introduced into his evolutionary scheme by incorporating Breuil's new evidence. But why was he too committed to the epochal "paradigm" to revise it? An easy way out existed -- one could merely assume, as Breuil himself apparently did, parallel phyla of cultural evolution to match those in the realm of morphology.[93] Part of the explanation for MacCurdy's failure to do so may lie in an inherent conservatism of thought; but part must also be a consequence of his conception of the process of culture growth, a conception that ranks on its own as an important theme in his writings.

As we have seen above, MacCurdy was a committed and throughgoing cultural evolutionist, who tried to trace not only the evolution of stone tool types, but also that of the "complexes" of culture traits that could be inferred from the archeological record.[94] Despite his assertion of a cultural break between the Middle and Upper

Paleolithic, when he wrote about the growth of culture as a whole he tended to characterize it as a nearly continuous process of accretion in certain key areas of cultural attainment -- hunting, religion, art, etc. Thus, because he had a strong tendency to portray human culture as a single entity evolving over time, he would have resisted suggestions that there had been separate paths of cultural evolution, even while he could accept an analogous picture when considering the creatures who had made the culture.

This "schizophrenic" position becomes even more understandable when one notes that the basic explanatory principle he invoked in this saga of cumulative accretion was psychological rather than biological . That is, instead of integrating the evolution of Paleolithic culture with the process of biological change that hominids had undergone up to and including the emergence of *Homo sapiens* he usually explained advances in culture as the product of a psychological quality, the "inventive faculty" or human "inventiveness."[95] When in addition, he characterized culture as a sort of "racial bank account" whose mounting interest resulted in increasing human control over the environment, the impression that there was some sort of non-evolving "human mentality" behind the evolution of culture was strengthened.[96]

A pair of examples from MacCurdy's pre-World War I writings shows this tendency to "psychologize" fossil hominids at work. In a 1905 discussion of the "Eolithic" epoch, he sketched the following picture of the passage from the "Eolithic" to the Paleolithic: during the "Eolithic," he said,

> the requirements in the way of tools being very simple and the supply of materials [i.e. naturally occurring flint flakes] being very plentiful, the inventive powers of the population remained dormant for ages ... [Later] the stock of tools increased slowly with the slowly growing needs. As these multiplied, and the natural supply of raw material diminished, the latter was supplemented by the manufacture of artificial flakes. When the lesson of associating definite forms of implements with definite uses was learned, special types arose. Then came the transition from the Eolithic to the Paleolithic.[97]

A similar idea also appeared in his first discussion of the Piltdown problem; there he wrote that he had suspected the discovery of early man in Sussex flint deposits, because "of all raw materials flint is perhaps the best suited to tempt nascent *Homo* to become a tool user."[98] The important thing about both passages is that their terminology is non- or even anti-evolutionary; a Lamarckian or a selectionist process, it is true, *could be* invoked to explain the proposed changes in cultural capacity, but none *was* invoked, and left alone, the language was completely consistent with the assumption that no organic or behavioral evolution was involved at all. Mere learning was sufficient to explain the changes described, and there was no indication at what point in the evolution of culture more than learning had to be invoked to explain change.

That this "psychologizing" habit was not a passing fancy is clear from examples of MacCurdy's work during the 1920s -- in passages that dealt with the general problem of the evolution of culture rather than specific "epochs." Thus in the analysis of the growth of "culture complexes" which appeared in Human Origins he asserted that "man has conquered the terrestrial domain because of his inventiveness, his ability to harness external forces," and especially sources of "external energy" like fire.[99] While he tried to illustrate the successive steps in human control over the environment with his theory of the "primary," "secondary" and "tertiary" tool types, he failed to provide an explanation of how the all-important faculty of "inventiveness" had evolved.[100] The way was thus open, whether he intended it or not, to assume that this faculty had been a constant.

MacCurdy confused the issue still further in another exposition of the "three stage" idea that he published in 1926. There he spoke of human inventiveness as "a more efficient accelerator" in cultural evolution than mutation was in physical evolution, an "accelerator" because inevitably "one invention leads to another by a system of branching and budding, so that a single invention may give rise to a whole cluster of related activities."[101] Invention, he claimed as well, was the product of an elite, of a few "exceptional minds capable of arriving at a goal by indirection;" the "course of progress" was thus determined by these few, while the "rate of progress" reflected the "ability of the many to profit by the achievements of the few."[102]

There were two major intellectual problems involved in this formulation. First, its elitism made it almost static in an evolutionary sense; it replaced the advance over time of whole populations with an apparently constant number of geniuses whose presence made it necessary for the vast majority to advance only in some sort of imitative faculty. It also tended to disassociate the biological intelligence of the hominid population from its cultural progress, since inventions themselves seemed inexorably to imply other inventions within a given cultural "complex." Cultural and brain evolution thus seemed to lack any clear connection. If applied to populations of anatomically modern <u>Homo sapiens</u> this kind of thinking would have mirrored the perspective of prominent cultural anthropologists of the time like Franz

Boas;[103] however, in relation to earlier hominid groups it seemed to work at cross purposes with the scenario of gradual enlargement of the brain and increasing intelligence that MacCurdy endorsed.

While passages like those above dominated MacCurdy's general discussions of the evolution of culture, this is not to say that he always left physical and cultural evolution on separate tracks in his work. When trying to demonstrate the "primitive" level of intelligence of morphologically "primitive" fossils, he definitely tried to correlate the two. We have already seen this attitude at work in MacCurdy's handling of the Neanderthal question;[104] he also asserted, both in Human Origins and elsewhere, that the level of brain development (as read from endocranial casts) of earlier Pleistocene hominids like Piltdown man and Java man closely matched the crude level of human cutlture that had supposedly existed during their respective geological epochs.[105] Still, these attempts at correlation fell far short of resolving the uncertainties of his theory of cultural evolution, since they merely described associations in mental and cultural level within various hominids, and differences in level among them, without attempting to account for either.

When viewed as a whole, then, MacCurdy's view of hominid evolution was an inconsistent one -- being unilinear on the evolution of Stone Age culture, multilinear on the biological evolution of fossil hominids, and vaguely unilinear or possibly non-evolutionary on behavioral issues like the emergence of the hominid adaptive pattern and the role of inventiveness in culture growth. What was necessary to clarify and refine his conception was a thing that he apparently could not provide, that is, a "biocultural" theory that could not only correlate morphological and cultural changes but also explain them as instances of the operation of general evolutionary forces like adaptation to environment, natural selection, and so on. And if human behavioral evolution were a special case which possessed aspects that required other explanatory principles, the respective areas in which biological and non-biological theories applied would also have to be marked out.

It would be unfair and not really relevant to berate a past scientist for failing to achieve something that many might say still eludes present day researchers. Also, if it is immensely difficult to explain why an individual espouses a particular theory, it becomes an even greater puzzle to explain why that individual never thought of a particular alternative viewpoint. Even so, one can identify several factors in MacCurdy's intellectual environment that would have inhibited a search for the kind of "biocultural" theory described above.

First, in an era in which resistance to the idea of human evolution was still strong, the balance between straight description and the design and testing of theories might be expected to lie on the side of description. The need to demonstrate through careful accumulation of descriptive detail the fact that humans had evolved would thus assume first priority; launching new, and speculative theories, especially in general works addressed to the educated public, might have been seen by a cautious writer as impairing the aura of scientific sobriety that one needed to maintain. Probably more important, though, was the fact that biological theory during the period between 1900 and 1930 could not really offer the requisite general evolutionary principles. As we have seen in the case of Osborn, paleontology, to which a student of fossil hominids would have looked first for guidance, offered mostly non- or even anti-Darwinian versions of "orthogenesis" and "irreversibility," that is when it was not trying to remain strictly "empirical" and hostile to general theory.[106] Also, the fields of research that would eventually provide the unifying principles of the "new synthesis," such as population genetics, were still in an embryonic state. Finally, the state of tension that existed between biological and cultural anthropology in America inhibited meaningful interchange on an issue that concerned both disciplines.[107]

Within the discipline of paleoanthropology itself, the greatest barrier to an integrated theory of human physical and cultural evolution was the misleading character of several of the key pieces of evidence that were generally accepted at the time MacCurdy was writing. The "chimaera" of Piltdown man was an obvious problem, as has been noted so often since the fraud was discovered.[108] Even if one rejected the jaw, and subjected the endocranial cast to an analysis that "proved" the primitiveness of the Piltdown brain, one still was at a loss to provide a coherent functional account of the great differences in skull form between Piltdown and other "primitive" fossil hominids. Boule's "hatchet job" on the Neanderthals had also created a body of data which seemed to require a long period of parallel evolution separating this hominid line and the one ancestral to modern _Homo sapiens;_ this was a scenario that only a highly speculative interpretation of the fossil record and a strained account of the archeological record could produce.[109] In addition there was an unfortunate gap in the evidence regarding the earlier phases of the Paleolihic. While the "Chellean" and "Acheulian" cultures had been extensively documented, there were no fossil hominids connected with them prior to the 1930s. It is now generally accepted that two of the authentic fossils of MacCurdy's day, "Homo heidelbergensis" and "Pithecanthropus erectus" belonged to a single hominid species, Homo erectus, which produced implements in both these tool traditions; as we have seen, however, the morphology of these forms had led MacCurdy, and others, to consider Java man especially as too "primitive" to have been Paleolithic tool workers.

These problems of evidence were compounded by dubious methodological assumptions and theoretical expectations. The dangers implicit in typological thinking and the use of racial analogies have already been noted. The habit of making psychological inferences based upon details of form in endocranial casts allowed scientists to launch fanciful reconstructions of a fossil's mentality. The great pressure commonly felt to push primitive-looking fossils off the main line of human evolution, and to hypothesize the early appearance of anatomically modern forms of humanity has been mentioned in the context of Osborn's work as well. Finally, there was the closely related expectation that the brain had been the leading factor in human emergence. Not only did this expectation work against the acceptance of "small-brained" hominids like Java man as human ancestors, it also lent accounts of the appearance and initial development of the hominid line a curious self-starting quality. Because these creatures seemed to evolve largely from the needs of their already advanced brains, the search for external factors in the process was inhibited.

1925-1935: Adjusting to New Fossil Discoveries

While this attempt to explain MacCurdy's limitations as a theorist reveals a complex pattern of influences, one major effect of these limitations is quite easy to trace. Simply put, because of his angle of vision on human evolution MacCurdy was in a poor position to understand the significance of the major fossil finds of the 1920s and 1930s. An important example was his interpretation of the discovery that over the years has captured the greatest share of public attention, *Australopithecus africanus.* Raymond Dart, the initial describer of *Australopithecus,* though at first unwilling to place it in the human family, had stressed the hominid-like characters of the creature, and of its teeth and brain in particular;[110] his claims, however, ran counter to the belief in the early appearance of large-brained Homo that was part of the most common interpretation of multilinear evolution. Thus when Sir Arthur Keith questioned the significance of Australopithecus for human evolution, arguing that it retained too many "ape-like" features at a probable geological date when "man was already in existence," MacCurdy went along.[111] Thus, the fossil never received a place in the latter's account of hominid phylogeny.

Similar difficulties arose in the interpretation of what was perhaps the most dramatic discovery of the interwar years, that of Peking man. In this case the hominid status of the fossils was beyond doubt, but MacCurdy seriously underestimated the cultural capacity of "Sinanthropus" in order to preserve his previous estimate of the mental level of early Pleistocene hominids. Thus, in a 1930 article describing recent work in prehistory, he stressed the fact that no cultural remains had yet been found in the "Sinanthropus" deposits at Choukoutien; by citing the "thousands of cubic meters" of material that had been sifted he also implied that none would be found.[112]

His near certainty on this point surely had resulted from two considerations: first, that Peking man was a creature generally perceived to be close in evolutionary level to the supposedly cultureless "Pithecanthropus," and second, that the "lower Quaternary" date provisionally assigned the former fit a creature with a mentality bordering on the "Eolithic."[113] Later, when additional thousands of cubic meters had revealed a culture that would eventually be considered part of a major tool tradition nearly as advanced as the Acheulian,[114] MacCurdy would only allow that "the race had a certain facility in chipping stone implements and knew how to make use of fire,"[115] achievements that were "not much, if any, above the cultural stage reached by Piltdown man."[116]

While it was unfortunate that MacCurdy missed the significance of both the australopithecines and Peking man, he lost his greatest opportunity to participate in a reevaluation of accepted theory on another issue entirely, the interpretation of the Mount Carmel finds of the 1930s[117] -- for in this case MacCurdy, as head of the American School of Prehisroric Reseach, had played an important role in bringing the fossils to light. Mount Carmel was in Palestine, which in MacCurdy's view was a good place to to look for hominid remains, since it constituted a veritable crossroads between Europe, Asia and Africa.[118] Palestine had started to fulfill its promise in 1925, when a British scientist found a "Neanderthaloid" skull in the Galilee region. MacCurdy's visit to that site excited his interest, and when in 1929 the British archeologist Dorothy Garrod invited the American School to undertake joint excavations, he agreed.[119]

In seven seasons of work at Wadi el-Mughara (Valley of the Caves) under Garrod's leadership, the joint expedition uncovered an impressive series of cultural remains -- a series which according to MacCurdy documented "practically every epoch" from the "Tayacian" (Breuil's term for a particular Lower Paleolithic flake industry) to the Bronze Age.[120] But even more significant than the tools were the fossils that were unearthed in two of the Mt. Carmel caves -- Mugharet et-Tabūn (Cave of the Oven) and Mugharet es-Skhūl (Cave of the Kids). Since the major specimens were all found in what Garrod considered a "lower Levalloiso-Mousterian" context, a variant of the Middle Paleolithic related but not identical to the "typical" Mousterian of Europe, MacCurdy felt justified in labelling the Mount Carmel population as "Palestine representatives of Neanderthal man."[121]

For one of the Mount Carmel fossils, the female skeleton found in the Tabūn cave, the "Neanderthal" label was reasonably accurate. Though it was less pronounced in some of its characters than the "classic" Neanderthals of western Europe, it fit the general "type" well -- retreating forehead, chinless lower jaw, short, robust stature, etc.[122] Other specimens, however, presented anomalous characters that should have proved unsettling to the notion that the entire population could be characterized as "Neanderthals." In the Tabūn cave itself, a large lower jaw was found 90 centimeters beneath the Tabūn "woman," but it was a jaw that appeared to have a prominent chin, a most un-Neanderthaloid character. MacCurdy tried to circumvent the problem by arguing that individual differences, in this case "bordering on abnormality," seemed to be the "logical explanation" for the contrast between the two Tabūn mandibles.[123] If the fossil with a chin could be taken as the "abnormal" one, then the Neanderthal tag would still be safe.

If Tabūn presented one problem, then Skhul revealed a whole collection. During the excavation of the latter cave, which was supervised by Theodore McCown (1908-1969), a young physical anthropologist connected with the American School, parts of at least ten skeletons came to light.[124] After painstaking preparation of the fossils, McCown, in collaboration with Sir Arthur Keith, made a morphological examination which yielded surprising results. Despite well developed brow ridges and robust facial structures that approximated "Neanderthal" conditions, the Skhūl population also contained individuals whose skulls displayed important "progressive" characters alien to the Neanderthal "type," such as high foreheads and well rounded occipital regions.[125]

Not only did the Skhūl population show a composite morphological pattern that cut across the boundaries of traditionally defined "types," its individuals also varied greatly among each other, so much so that MacCurdy, following Keith and McCown, asserted that if they had been found in different places they would have been assigned to "more than one variety of a common extinct race of mankind."[126] And of course, with the Tabūn individuals added in, the range of variation in the Mount Carmel group as a whole was much wider. The presence of so many "progressive" characters had to appear all the more unusual because Garrod had dated the fossil bearing deposits of both caves as "Riss-Würm" or Third Interglacial, and thus as older than the major "classic" Neanderthal fossils, which were attributed to the early Würm, or Fourth Glacial period.[127]

With the changes in anthropological perspective that have come about since the 1930s as a guide, it is easy to point out several questions arising from the "facts" about

Mount Carmel as MacCurdy knew them, questions that would have proved disturbing for the theories he had developed prior to the discoveries. First, given the wide range of variation that could occur in a skeletal sample from a single site, was it wise to accept racial "types" of fossil hominid that were based on a handful of individuals, or even a single supposedly "representative" specimen? Also, did the occurrence of quite different forms of fossil hominid -- e.g. the Skhūl population and the "classic" Neanderthals of Europe -- in similar cultural contexts cast doubt upon the practice of correlating cultures with specific racial "types"? Finally, did the association of quite modern-looking braincases and "Levalloiso-Mousterian" tools at Skhūl specifically undermine accepted theories about the gross mental level of the Neanderthals, since the latter also utilized a similar technology?

Since, as we have seen, MacCurdy was the sort of person whose commitment to received ideas was strong, we should not be surprised that these questions did not occur to him. Rather than reevaluate long held beliefs, he merely stretched his categories to receive the new evidence. He acknowledged the major factual anomalies. The Tabūn "woman" was indeed much nearer to "what has been looked upon as the Neanderthal type" than the Skhūl "people." While "actually older than the Neanderthalians of western Europe," the Skhūl fossils did "stand somewhat closer to Neanthropic man [the group ancestral to modern Homo sapiens] in a morphological sense." Once these qualified admissions were made, the Mt. Carmel fossils were nevertheless ready to take their place as Palestine's "Neanderthals."[128] And perhaps in order to forestall the messy questions about ancestors and descendants that "progressive" Neanderthals might raise, MacCurdy was quick to quote the preliminary conclusion of McCown and Keith that the Mt. Carmel group were "unlikely" to have been the progenitors of any modern humans.[129]

Intruiging as the Mt. Carmel finds were, they would thus not shake the belief in the replacement of the Neanderthals, or deter him from the search for the true ancestors of present day humans, who would probably be found "somewhere in Asia."[130] His commitment to parallel phyla, the mental inferiority of Neanderthal man, racial typologies, and the idea that cultures functioned as racial markers proved too powerful to allow him to consider the possibile implications of the new data.

Mt. Carmel, then, was a lost theoretical opportunity as far as MacCurdy was concerned. That this was so might have stemmed from a generally conservative mental outlook, but his reaction to the major discoveries of the interwar years also presents a picture of what one might call, in imitation of Kuhn, an intensely "normal" scientific mind at work. Such an individual makes a strong effort to keep abreast of new developments and to make what practical contributions he can to the progress of his discipline. But new "facts," even those that he has a personal role in discovering, are to be handled within the accepted "paradigm," as modified by his personal choice of subsidiary theories. Early in his career there may be a period where competing theories are evaluated and a personal viewpoint hammered out. But once a set of mutually reinforcing ideas (or at least a set with that appearance) is accepted as the best available, the process of examining basic assumptions nearly stops.[131] It seems clear that by the mid-1920s MacCurdy was well into this theoretically quiescent phase of his career.

The last two "themes" that we shall look at in MacCurdy's work -- his acceptance of "eoliths" and his espousal of the "magical" interpretation of Paleolithic art -- illustrate these characteristics of MacCurdy's mind quite well. He settled upon his interpretation of both issues relatively early in his career, when both were matters of archeological controversy. Eoliths remained controversial, while the view of Paleolithic art that he espoused hardened into orthodoxy. Yet in both cases, once he had worked out an interpretion that fit in well with his general conception of the evolution of culture, his own view changed little, if at all. Though they relate closely to other themes analyzed above, they deserve to be discussed independently because of the great importance they assumed in MacCurdy's output, and because he knew and wrote more about both issues than any of his American contemporaries.

The Importance of Upper Paleolithic Art

One does not have to seek far to discover how deeply impressed MacCurdy was by the art of the Upper Paleolithic. He included detailed discussions of the most recent art finds in his periodic reviews of prehistoric research,[132] and sought to analyze the significance of Upper Paleolithic art in articles aimed at both academic and general audiences.[133] Explicit statements about the impact of prehistoric art on MacCurdy's thinking are not easy to find, though; unlike Osborn, he apparently did not think the public or the scientific community deeply interested in his own reasoning processes and personal experiences.

Still, some of MacCurdy's remarks about particular sites and art objects do reveal personal reactions -- for example, this one about the famous sculptured bisons of the Tuc d'Audobert in France, which was among the sites visited by the students of the American School for Prehistoric Studies in its very first summer of existence: "the two bison figures modeled in the clay on the cavern floor and almost completely in the round are in turn stupefying, bewildering, and admirable. This group represents more nearly than any other one thing the sum total of the cave man's mode of thought and life."[135] This choice of words to describe what obviously was a moving experience unintentionally brought out a serious problem that arose from giving art a prominent place in one's interpretation of prehistory: how could one really claim to know a whole mentality and way of life from objects that were so "bewildering," and difficult to explain?

MacCurdy, in fact, enhanced the difficulty of interpreting Upper Paleolithic art by insisting on its <u>uniqueness</u> -- a quality which he surely had in mind when he used the adjective "stupefying" to describe the bisons of the Tuc d'Audobert. He believed that it differed from other forms of primitive art produced by both Neolithic and modern "preliterate" peoples in several crucial ways. All the ways, though, could be summed up in a single key contrast -- i.e. between the <u>naturalism</u> of Upper

Paleolithic art and the conventionalism, or schematic quality, associated with other types of primitive art. It was this contrast that he had in mind when he asserted that[136]

> The art of the untutored child is more like that of neolithic or modern primitive art [sic] than it is like paleolithic art. The child does not copy the thing itself as much as his ideas about the thing. Paleolithic art evinces a remarkable familiarity with the object combined with a skilled hand.

As evidence for the generalization that Paleolithic art valued "the real, the natural" over "the mythical, the artificial"[137] he cited the following "facts." First, Paleolithic artists chiefly portrayed game animals, rendering them with a level of realistic detail that showed close observation, supposedly far closer than in the animal representations of other primitive artists. Second, while "mythical representations" abounded in other primitive art, Paleolithic art revealed almost no images that could be easily interpreted as supernatural beings like gods or mythical animals. Finally, the images of human beings in the Upper Paleolithic, even though clothing was undoubtedly worn during the period, stressed realism by their almost exclusive use of the nude female figure; primitive art, however, and even that of early civilizations like Egypt, was analogous to the art of children in picturing the human figure conventionally clothed.[138]

Whether or not these observations were accurate, and they were only partly true at best,[139] one inference that MacCurdy drew from them was quite significant. While he considered other primitive art as possessing a "child-like" quality, the art of the Upper Paleolithic represented to him a fully mature use of human imagination. This attitude comes though clearly in the following analogy:[140]

> Without a background of art inheritance and beset by insuperable difficulties, the troglodyte artist left a record, of which any age might well be proud. ... If France has her Louvre, she likewise her Font-de-Gaume; and the art student who would visit the Prado Museum in Madrid should not fail to include the Quaternary gallery at Altamira.

Though he never made the comparions explicitly, this emphasis on the high quality and mature "naturalism" of Upper Paleolithic art would surely have reminded educated readers of classical art. Anyone who read MacCurdy would thus be well prepared for Osborn's celebration of the Cro-Magnons as "Paleolithic Greeks."

To portray prehistoric art as such a transcendant achievement placed a burden on anyone who hoped to explain the phenomenon. Perhaps the simplest way around the problem was to infer noble motives that would match the nobility of the product. It is not surprising, then, that early interpreters of Ice Age art explained it largely as the product of an inborn love of beauty. "Art for art's sake" is the motto that modern critics have applied to this theory.[141] Indeed, in Man Rises to Parnassus, Osborn ascribed such a motive to the cave artists -- the so-called "art impulse" of the Cro-Magnon "race."[142] MacCurdy also approached this viewpoint at times, for example when he asserted that "man was artist ... before he was the maker of even hieroglyphs. He tamed his imagination and his hand to produce at will objects of beauty long ages before he tamed the first wild beast or made the humble plant world do his bidding."[143] He struck a similar lofty tone when he proclaimed that the emergence of artistry represented a sort of quantum leap in mental power above previous human abilities, for the works themselves were "so skillfully executed as to be of genuine merit, stamping the caveman as something more than mere artisan, his goal something beyond the merely utilitarian."[144]

MacCurdy was not really satisfied, however, with using perhistoric art as an indicator of a mature artistic sense alone. In his earlier work he tried to underscore the importance of art in the Ice Age by advocating a theory of the late 19th century archeologist, Louis Piette (1827-1906), who held that many Upper Paleolithic images were in actuality linguistic symbols. If this were so, said MacCurdy, it provided proof of the full intellectual maturity of early Homo sapiens, for this "ability to clothe ideas with perpetuity" and the freedom of the hands for tool-use were the great "lever and fulcrum that have lifted man higher than the common animal plane ... From pictograph to alphabet is not a long step."[145] As useful as the theory seemed, however, he did not emphasize it in his writings after about 1914, for Piette's theory failed to find favor among archeologists of MacCurdy's generation, and was eventually overshadowed by other, more comprehensive theories of Upper Paleolithic art.

MacCurdy had an evident need for a more general theory explaining Paleolithic art. Even retention of the symbolic theory would only have taken care of the most schematic and standardized data -- those images that could be subsumed under the category of "pictographs." The meaning and social function of the most dramatic forms of representational art, the cave murals and sculpture of the Ice Age, would have remained obscure. Finding such a theory, though, was difficult. Analogies with the western artistic tradition could not provide a satisfactory model. Life in the caves of France was obviously vastly unlike that in the city states of Greece. The problem was compounded by his belief that the art of modern preliterate peoples could not be profitably compared with

Paleolithic achievments. Thus the time honored "comparative method" whereby present day primitives served as stand-ins for prehistoric man could not easily be invoked.[146]

Ironically, the theory that he settled on -- the magical or religious interpretation of Paleolithic art -- was one that had originated through the use of ethnographic analogies.[147] Yet when MacCurdy outlined his version of the theory, he was able to present it as the result of internal iconographic evidence alone.[148] He could do so because the ground work for such an analysis had already been laid in Europe, first by the anthropologist Solomon Reinach (1858-1932),[149] and then by the man who was emerging in the years around 1910 as the greatest authority on Paleolithic art, the Abbe Breuil.[150] The first indications in MacCurdy's writings that he was beginning to see cave murals as evidence of "sympathetic magic" -- in this case as key elements of rituals to guarantee hunting success and an abundance of game -- came in 1910.[151] Full treatments of the magical theory did not appear till the mid-1920s,[152] and by then it was clear that the magical interpretation had become the cornerstone of his understanding of Paleolithic art.

The theory that the people of the Upper Paleolithic looked upon art as a magical way of affecting events in the real world rested upon several "factual" observations about the imagery of the art objects themselves and the contexts in which they had been found. First, the artists tended largely to depict game animals, and allegedly made such a close observation of their objects that exact rendering of the animal was an obvious and major goal. Second, the frequency with which a species was depicted in a particular cave seemed to be proportional to its representation in the bone refuse of that cave, especially if it was a large species of mammal that might be difficult for the Paleolithic hunter to procure -- e.g. the horse, the mammoth or the deer. Also, the fact that murals tended to occur in hard to reach portions of caves seemed to indicate that a ritual rather than decorative motive was involved in their production. Finally, in species where the sexes were easily distinguished from each other, it seemed that the artists generally portrayed female animals.[153]

For MacCurdy, as for Breuil and Reinach, all these characteristics pointed to a single conclusion, that most depictions of animals in cave art were "prayers for the increase of the species useful for food."[154] Other, less frequent types of imagery tended to confirm this "votive" function for art -- such as the depiction of animals with straight lines suggestive of spears or arrows drawn through them, or animals whose kneeling or reclining posture suggested that they had been brought down in the chase.[155] In addition, though human figures were much less common than animal, they also appeared to have functioned in a kind of fertility magic, since the dominant human image was a female of the so-called "Venus" type, which tended to exaggerate the female sexual characters.[156]

If Paleolithic art were as unique a phenomenon as MacCurdy alleged, and if it could be seen as largely magical in function an obvious question arose -- what causes had made it appear when and as it did? His speculations on this subject were interesting, since they constituted his only attempt at an ecological explanation of the emergence of a major human cultural or physical characteristic. Specifically, his account revolved around the supply of game available to the human population. In the Upper Paleolithic the density of human settlement had, he believed, increased because of the greater efficiency of human hunting practices over those of "Mousterian man." This change had brought in its train a "corresponding decrease of game," and the human response to the latter development was allegedly the invention of art and the subsequent wide use of artworks as "votive offerings for the multiplication of game and success in the chase."[157]

If the iconographic analysis of the art that underpinned the "magical" theory had been accurate,[158] there would have been much that was plausible about this ecological explanation for the phenomenon. Given the fact that Upper Paleolithic industries exhibited a range of hunting-related tools -- bone harpoons, spear throwers, etc. -- more extensive and sophisticated than that available in the Middle Paleolithic, it appears reasonable that a more effective use of resources would have become possible. This, in turn, could have spurred an increase in human numbers, and an eventual overexploitation of game.[159] Still, all that would have been necessary to raise plausible speculation to the level of cogent theory was lacking. For example, MacCurdy could point to no studies that tried to measure the relative hunting success enjoyed by Middle and Upper Paleolithic populations, nor to any studies that attempted to estimate the densities of these populations. Without these kinds of data, only weak inferences from an increased inventory of tool types were available to support his explanation.

It would obviously be ahistorical to expect MacCurdy to have undertaken such studies with all the methodological tools now available to paleodemographers and paleoecologists. Nevertheless, as with Osborn,[160] one must ask why he never recognized the need for at least rough and ready approximations of such studies. In MacCurdy's case the explanation seems clearer, though. It appears to stem from the conception of the relevant scientific problems or "puzzles" that working within a particular archeological "paradigm" had given him. In the context within which he worked the professional labor of collecting precise data and testing hypotheses seemed to be necessary mainly for the tasks of identifying Stone Age tool "types" or artistic styles and motifs, fixing their geographical distributions and sequences of succession in time, and relating them to glacial chronology. Major theoretical questions of cultural evolution -- such as that of the origin and function of art, or the function of various tool types -- seemed to represent interesting, but subsidiary issues for which more speculative treatment sufficed.[161]

Had he been confronted with such a characterization of his priorities, MacCurdy would probably have responded that in the instance of Paleolithic art detailed ecological and demographic analysis was unnecessary; his inferences were logical and even elementary, given the clear "proof" provided by iconographic analysis. Actually, though, the message conveyed by his own description of the data was not so unequivocal, for while he portrayed a gradual evolution in artistic styles and techniques, he also claimed that the types of images and motifs employed had remained uniform from the Aurignacian through the Magdalenian "epoch."[162] Such stability of imagery would seem to suggest a similar uniformity in the function of art right from the commencement of the Upper Paleolithic, yet MacCurdy's theory made it seem as though art was the outcome of environmental stresses that would not have appeared till some time later, and whose intensity would have increased over time. In addition there was the possible inconsistency presented by the "Venus" statuettes -- i.e. would man have prayed for human fertility, when his "hunting magic" showed that he was already pressing on the avilable food supply with his current numbers?

If MacCurdy's evidence on the relation between art and its environment was weak, there was also a paradox in his reasoning, a paradox that reduced the value of art as a criterion of the advance of human intelligence. As we have noted above, MacCurdy tried to measure the evolution of the human mind through the evolution of culture. In his clearest description of the latter process he asserted that the growth of culture was marked above all by improvements in human control over the external environment, especially improvements that harnessed energy to achieve human aims. The hunting and fertility magic involved in Paleolithic art would, however, have brought no gains in this crucial area; in fact, one could argue that they would have set back culture's ability to adapt to and control environment, since magic misinterpreted the way in which human effort could solve environmental problems like the provision of adequate game supplies. Under these conditions placing the emergence of art-as-magic in a central place in one's portrayal of the Upper Paleolithic actually did little to support the hypothesis that the "Aurignacian race" was mentally far superior to its artless predecessors.

Despite its omissions and inconsistencies, MacCurdy's basic viewpoint on Upper Paleolithic art remained consistent from the period around 1910 to the end of his career. Because from the late 1920s on most of his scholarly interest lay with the American School's work in Palestine, his last decade of writings did not advance beyond his earlier work in the interpretation of prehistoric art. His only discussion of the subject was merely a compilation of the most recent finds.[163] Even in his most extensive treatment of the question, the long chapter on Paleolithic art in Human Origins, it was significant that most of the key explanatory passages were direct copies from earlier writings.[164] The only major new effort undertaken was a characteristic attempt to catalogue fully the many sites, subjects and styles so far uncovered in Europe.[165] In the area of Paleolithic art as in his discussion of other aspects of human evolution, the later stages of MacCurdy's work merely presented an attempt to fit new data into a theoretical structure that had hardened early.

MacCurdy and The "Eolith" Problem

The last theme to be discussed -- MacCurdy's defense of "eoliths" -- presents just as striking a picture of stability as his views on art. It was also highly significant in his professional career, both becasue it provided the content for his first major journal article in 1905,[166] and because he was the leading defender of "eoliths" in the U.S. until Osborn's conversion in the 1920s. More than anything else it is his commitment to this theory that makes him appear so antiquated to the modern eye. To be sure there were other theories that he defended -- the "magical" interpretation of prehistoric art, and the unilinear series of cultural "epochs" are the most important -- that modern archeologists would regard as "dated". But the former theory was at least fresh in the years when he took it up, and stood firmly on the best evidence and authorities then available. The latter, though a holdover from the late 19th century, had attained the status of a "paradigm;" while it would eventually be criticized and modified, even today it has not been completely superseded.

The "eolithic" theory had the virtues of neither of these two other theories, however. It was a generation old by 1905, and major authorities questioned the base of evidence which supported it. It was not a necessary part of the archeological "paradigm," but rather a convenient sub-theory that could be easily integrated into the former. However, it could just as easily be left out without serious harm. Yet MacCurdy had reasons for taking up the "eolithic" theory that seemed just as strong to him as those which caused him to support these other ideas, and one must examine them in order fully to understand MacCurdy's view of prehistory.

The basic assumption that underlay the belief in an "Eolithic" stage of culture was that of gradualism, i.e. the idea that in the evolution of culture as in that of life generally nature proceeded by steps small enough to approach true continuity of development. As MacCurdy's approach to the Mousterian-Aurignacian transition revealed, he did not always commit himself totally to the gradualist point of view. Yet even here it must be remembered that in his polyphyletic theory the line leading to Aurignacian Homo sapiens would have undergone gradual, progressive evolution in preparation, as it were, for the battle with the Neanderthals. Indeed, race conflict is a perfect way of reconciling apparent discontinuities with a gradualist evolutionary model.

As critics of gradualism have pointed out,[167] it has dominated evolutionary theorizing for most of the time since Darwin himself advanced it in the Origin of Species. Applied to the early history of culture, gradualism seemed to require a transitional stage between the tool-less anthropoid level and the known Paleolithic cultures, in which stone tools were struck to traditional and recognizable patterns. Specifically, that appeared to mean a stage in which hominid use of tools was present, but in which intentional fashioning of tools was either haphazard or lacking entirely. Simple psychology, MacCurdy asserted, established this viewpoint, for after all[168]

> the discovery that knives and forks were the best substitutes for teeth and fingers was not made in a single generation. It is safe therefore, to assume that it took the combined efforts of generations of eolithic experimenters to arrive at the idea of correlating a given form of tool with a given use of series of uses.

The gradualist inference that stages in tool use prior to the Paleolithic had to exist had strong appeal. Even a writer who reviewed MacCurdy's first article and was sceptical of much of the alleged evidence regarding eoliths had to admit that the earliest Paleolithic culture then known, the Chellean, represented a "grade of development in implement making too advanced to be considered as the first stage."[169] Indeed, that intuition has turned out to be accurate, since a more primitive tool tradition than the Chellean or Abbevillian has been discovered -- the Oldowan, or "pebble" tool tradition discovered by Louis Leakey,[170] though the latter tools would not satisfy the definition for "eoliths." In addition, such was gradualism's plausiblility that it was easy to grant the idea of an "eolithic stage" the status of an unprovable truism -- to accept it in theory but allow it no practical importance, because of the difficulty of identifying whether tool-like natural fragments of stone and bone had actually been used by hominids. This was precisely the way in which some sceptics of MacCurdy's day handled the issue.[171]

Of course, anti-gradualist challenges were possible as well, but significantly MacCurdy did not feel the need to respond to them in his own work. Perhaps he felt that the logical necessity of the "Eolithic stage" of culture was accepted by all participants in the debate. At any rate, a line of argument like the following would have cut at both the gradualist and associationist assumptions of MacCurdy's model: it is not intuitively obvious that stone tool-making must be preceded by stone tool use sans intentional manufacture. Perhaps some sort of vague standardization of technique was the path of least resistance for a hominid brain of limited complexity, since it would circumvent the need for the individual to reinvent the stone tool every time he or she encountered a tool-using situation. An attempt to duplicate patterns that had "worked" in the past would thus be a creature's first approximation to a stone tool industry. Were this so, the earliest part of the archeological record would still be biased toward stone tools with an imposed regularity of form, and not toward a wide variety of naturally produced cores and flakes that were only utilized by hominids.

Illusory or not, when MacCurdy took up the "eolithic" theory in 1905, it already had a long history of controversy behind it. As far back as 1867, the Abbe Bourgeois, a French prehistorian, had proclaimed the existence of pre-Paleolithic artifacts in Oligocene deposits at Thenay, but these and other finds in France had never attained general acceptance.[172] In his own initial discussion of the "eolithic problem" MacCurdy relied principally on less ancient specimens -- i.e. the later Pliocene and early Pleistocene "eoliths" collected in southern England by Benjamin Harrison and Joseph Prestwich, and in Belgium by Alphonse Rutot. It was Rutot who had the strongest influence, especially through the personal guidance he gave MacCurdy on the tour the latter made of the Belgian "eolith" deposits while he was researching his first article. The impact of that visit shows through clearly in the answer MacCurdy said he gave to a colleague who had doubts about whether Rutot's views were really coherent or not: "know him" asserted MacCurdy, "cover with him some, at least, of the ground he has covered, and the language he speaks will no longer sound strange and unfamiliar."[173]

The tone that MacCurdy adopted here -- of the true initiate in rapport with an almost mystic source -- was very significant. One hears echoes of it in the remarks of Osborn about his dealings with Piltdown man.[174] Statements like these show how far a personal examination of remains in the company of their guardians (and champions) could go to induce suspension of disbelief. Starting out as "outsiders" in Old World prehistory gave American scientists a useful initial position of neutrality in matters of controversy like "eoliths." Apparently, though, being brought in on the "action" was usually more important than remaining above the battle.

Once convinced by personal study at the major sites, MacCurdy put forward two sets of observations about the "eoliths" of Harrison, Prestwich and Rutot. The first set purported to demonstrate that the stones had in fact been used as tools by early man, while the second argued that "eoliths" had evolved over time toward the more familiar and recognizable types of Paleolithic artifacts. He built up his first group of conclusions with an analysis of the "eoliths" from the Pliocene deposits of the Kent region of England. The second relied on the evidence provided by Rutot in Belgium.

After examining the collections made in Kent and adding a group of stones he had found there himself, MacCurdy concluded that the Kentish "eoliths" owed their form to the following pattern of tool-using behavior. The primitive workman, not knowing how to process blocks of flint into usable tools, would first pick up flint flakes of a size and shape approximately suitable to his purposes. He would utilize the flakes' sharp edges and cast the stones aside when they became dulled. In some cases crude flaking akin to "retouching" along the edges would be attempted before the "eolith" was discarded. When the workman encountered an angular flake, he would often render it easier to handle by simply breaking off a corner. Although stones of a great variety of shapes were apparently used, many "eoliths" could be fit into a few broad classes of artifact "types", of which Prestwich had identified three, roughly similar to Paleolithic "points," "scrapers," and "hammerstones." Apparently some chance shapes had looked more "suitable" to the eolithic tool-users than others.[175]

The central problem in establishing a scenario like this was a simple one -- how could one show that "eoliths," whose basic shapes were admittedly produced by nature, had in fact been subjected to human use and not just flaked and chipped by further exposure to natural processes? The best evidence, of course, was a fossilized tool-user, but as of 1905 there were no Pliocene hominid fossils in England. MacCurdy had to rely on the stones themselves. He rested much of his argument for modification by man on the occasional appearance of specimens whose regular patterns of chipping were unlikely to have been chance products. One example he gave was that of a "scraper" in which chipping along two adjacent edges had been done from opposite sides of the flake. To expect that nature, unaided, could have bunched all the chips in a row on only two of the flakes' four edges, <u>and</u> that "she would reverse the flake before beginning on an adjacent margin," seemed to MacCurdy to ignore "all the rules of probability."[176]

Unfortunately, the latter conclusion was not so obvious as it appeared. Given two facts about the sample of "eoliths" available -- 1) that those which had been chosen by collectors were probably only the most regular-looking fragments found in large flint-bearing deposits, and 2) that stones with unusual chipping patterns were a minority of this minority -- it would seem that probability might <u>demand</u> a small number of stones chipped in arbitrary, but regular patterns. The question that MacCurdy had to answer was not whether such stones could occur, but whether a greater <u>proportion</u> of the total number of stones in a deposit had more regular patterns than one could expect on the basis of chance alone. The method of searching deposits for stones of the eolithic "type" rather than sampling deposits statistically guaranteed that the latter hypothesis would not be tested, however. Thus, what one might call the argument from "pattern" was weak.

Another, and perhaps stronger mark of human presence that MacCurdy believed to be preserved in "eoliths" was <u>eraillure</u> -- the French term for a small secondary scar on the bulb of percussion of a flint flake which was produced by muscular opposition to the rebound created as the flake was being struck from a nodule. According to MacCurdy's authorities on the subject, experiments involving mechancial fracturing of flint showed that only human flint-knapping produced this mark. Thus if eraillure occurred on "eoliths," and he claimed that it did, that would prove human use.[177] As convincing as this argument was, there was a problem in the examples MacCurdy called upon to illustrate it. He produced only two such examples, and both were from deposits quite late within the "Eolithic stage." One was from the so-called "Cromer Forest Bed" of England, a supposedly "Upper Pliocene" deposit which is now known to be much later in date;[178] the second was not from England, nor was it Pliocene. It came fom the "Mesvinian" stratum of Rutot, the closest "Eolithic" layer in both time and tool types present to the start of the Paleolithic "Chellean" epoch. Rutot considered the "Mesvinian" to date from the early Pleistocene.[179] Specimens of flint from both these strata, it turns out, have been identified in the years since MacCurdy wrote as human artifacts, but as Lower Paleolithic ones.[180] Thus it is quite possible that he was right about the presence of eraillure but wrong in counting his examples as "eoliths" at all; and even if these few "eoliths" were what they were purported to be, this would not affect the status of the vast majority of stones, and all of the older ones, which did not possess this mark of human manipulation.

The "eoliths" from Kent that MacCurdy analyzed all had come from supposedly "Tertiary" deposits, specifically from sites designated as Pliocene. He took them, however, to be representative of "eoliths" generally, since, as we have noted earlier, he saw little evolution of tool form during the entire "Eolithic stage" of culture.[181] In regard to the important question of how far back in time "eoliths" could be found, he was willing in 1905 to credit some from Miocene deposits in France, although he had not examined these himself.[182] Not only did he leave his discussion of the artifacts of the earlier "Eolithic" era vague, but he also neglected to discuss a critical issue that worried at least one of his readers:[183] could the "primitive" forms of prehumans that must have been the only ones extant as early as the Miocene have been intelligent enough to use tools on such a widespread basis as the "Eolithic epoch" theory demanded? One could easily have asserted that the fossil record was just too fragmentary to allow useful debate on this question.[184] Merely to pass over the problem, however, did not constitute a solution that would win converts.

It seems probable that MacCurdy did not provide a more searching discussion of the earlier phases of the

"Eolithic stage" at least in part because he believed his conclusions about its closing phases to be so strong. This was where the studies of Rutot had proved so compelling. Working in the Pleistocene river valley terraces of Belgium, Rutot had distinguished several successive strata containing "eoliths," and had designated each stratum as a separate cultural "epoch." The first two of the latter -- the so-called "Reutelian" and "Mafflean" -- were typical "Eolithic" industries, differing from each other only in stratigraphy and not in tool typology.[185]

The upper two layers -- the "Mesvinian" and "Strepyan" -- showed novel features, however, features which indicated a transition from the "Eolithic" to the Paleolithic stage of culture. The "Mesvinian", which had an approximately First Interglacial date, still seemed to Rutot predominantly "Eolithic" in character, but differed from the earlier "epochs" because a much larger proportion of its flint tools owed their shape to artificial working rather than natural processes. The "Strepyan", though, which Rutot placed in the early part of the Second Glacial, showed major changes. First, several tool types that it shared with the "Mesvinian" supposedly showed "a gradual evolution in form" toward more definite and standardized patterns. More important, within the

"Strepyan" itself two kinds of crude "eoliths" -- "hammer stones" and "sub-cylindrical flint nodules" -- had udergone a gradual transformation, and by the close of the that "epoch" had become primitive forms of two characteristic Chellean-Acheulian tool types, the amygdaloid "hand axe" and the "hache" or "poinard."[186]

MacCurdy attached great significance to these findings of Rutot, for they appeared to document the existence of a "transition industry between the Eolithic period representing a low plane of mentality reflecting practically no industrial development and the Paleolithic period, signalized by a gradual evolution both mechanical and mental."[187] Since the Strepyan demonstrated step by step progress between the "Eolithic" and Paleolithic stages it supported a gradualist view of cultural evolution, and rendered the "eoliths" themselves more believable by anchoring them to universally recognized Paleolithic tool types. Placing the "transitional industry" wholly within the Pleistocene produced a mix of gradual evolution with a clear dividing line in <u>rates</u> of progress near the "Eolithic"-Paleolithic boundary. This mix became a central feature of the three-stage conception of cultural evolution promoted by MacCurdy in his later writings.

In trying to explain how this particular process of transition had occurred, however, MacCurdy fell into the same theoretical trap that would later mar his general discussions of cultural evolution. Instead of examining whether or how environmental pressures and opportunities might have encouraged natural selection for better tool-making capacities, or invoking an alternative evolutionary process that would produce advances in "mentality," he explained cultural change as the result of simple inventions. The best example was his account of the "hammerstone" - "hand axe" transition, which he portrayed as the outcome of successive discoveries made by trial and error. First, he argued, occasional hammerstones would receive blunt zig-zag edges from extensive pounding, and eventually someone would find these edges useful for cutting and scraping and thus try to produce them intentionally. Later, it would be discovered that intentional retouching could produce a straighter edge, and the use of thinner nodules or large detached flakes as starting points could provide a sharper one. When these latter practices had become habitual, the Acheulian "hand-axe" would have been the typical final product.[188]

That such a process of cultural change had occurred in the past was not necessarily implausible; the real problem was that MacCurdy described it in a way that focused solely on cultural evolution and left out physical and behavioral evolution entirely. It was thus a sort of Robinson Crusoe story, presented as though the main character were a modern individual member of Homo sapiens, who had merely been dropped into a situation where he or she had to create stone tools through trial and error. The evolution of ever higher "planes of mentality" was allegedly taking place, but the process of change depicted relied on purely psychological and non-evolutionary concepts.

There was at least one empirical difficulty in this account of Rutot's "transitional industry" that was just as important as its theoretical inconsistency. It revolved around a simple question -- even if one accepted some of Rutot's "transitional" tools, how did one know that they belonged with the "eoliths" that supposedly surrounded them? If one threw out the stones without signs of intentional shaping as not being artifacts at all, then one was left with a sample of tools of basically Lower Paleolithic style, only somewhat ruder or less complete than "typical" Chellean and Acheulian implements. The relatively recent date of the geological deposits in question enhanced this possibility. It would seem that only a belief in human utilization of the "eolithic" companions of these "transitional" tools served to place the latter within the "Eolithic" stage at all.

Despite these shortcomings in his evidence and arguments, MacCurdy continued to make faith in an "Eolithic" stage of culture a key element in his view of prehistory. His synoptic tables and reports of new discoveries continued to employ the concept,[189] and he produced a full review of the question in Human Origins. 1924 found him still to be a strong proponent of the authenticity of Tertiary "eoliths," and he defined the latter in basically the same way as before -- i.e. as tool "improvisations" whose basic form was provided by nature but which showed signs of utilization and/or intentional chipping by man or his ancestors.[190] In addition, MacCurdy appealed to much the same evidence in Human Origins as in his first article on the problem. While Osborn was making a great show of how Reid Moir's "Foxhallian" industries had transformed learned opinion on "Tertiary man" and his culture, MacCurdy merely integrated Reid Moir's findings into the picture he had already fashioned. He showed little willingness to abandon the evidence from England and Belgium that he had gathered in 1905.[191] In order to do this convincingly, though, MacCurdy had to deal with some important criticisms that had been raised against these "eoliths" in the meantime, and this defense is probably the most interesting part of the discussion of "eoliths" in Human Origins.

The fundamental question that had been raised against the geologically older "eoliths" was a simple one -- if natural physical forces, such as those produced by rushing water or ocean waves, were capable of chipping flints into characteristic "eolithic" shapes, then the presence of human tool-users could be ruled out by invoking Occam's razor. In 1907 an eminient group of prehistorians that included Marcellin Boule and Henri Obermaier subjected the hypothesis of stream action to a test. They used a type of centrifuge to pulverize rock samples from chalk deposits that often yielded "eolithic" flints. The result, wrote Obermaier, was that "we found ourselves confronted with typical eoliths ... forms with either partial or entire retouch around the edges, notched edges more or less deeply incurved," and so on.[192]

MacCurdy, however, disputed the notion that the "machine made" flints actually reproduced the real "eolithic" article. Instead he claimed that a German scientist had shown the former different in several respects -- for many possessed scars that did not result in actual flaking, many others had rounded corners, and or edges continuously chipped along one side.[193] This objection seemed strong to MacCurdy, but actually it was misconceived. The question was not whether a machine could create perfect "eoliths" as a matter of course, but rather if it could in a small number of cases produce an article which the eolith hunter would choose to collect were it found in nature. Whatever sequence of mechanical pressures had "worked" could then be presumed to have operated repeatedly over the broad range of time encompassed in a geological deposit; thus nature would have been able to produce as many "eoliths" as the most avid collector could want.

Another type of natural action that had been subjected to a test of sorts by skeptics was the pressure of geological strata on flint bearing deposits beneath them. In fact it was Henri Breuil who had found in a French gravel pit near Clermont (Oise) evidence of an apparent "Eolithic workshop," with flints revealing various stages of progress toward a variety of standard "Eolithic" types. The problem with this "workshop" was that it occurred in a basal Eocene deposit. To Henri Obermaier this seemed to be a reductio ad absurdum of the theory that the presence of "eoliths" proved the presence of tool-using hominids. Not only had the undisturbed nature of the deposit frozen the natural processes involved for identification, but the deposit itself was far older than those which contained the oldest known "anthropomorph" -- the diminutive fossil anthropoid

<u>Propliopithecus.</u>[194] By contrast, MacCurdy, who accepted the accuracy of Breuil's findings, could only say that it had become evident that "some of the earmarks hitherto looked upon as evidence of intentional chipping may be counterfeited by Nature."[195] This was not a strong defense, but as long as other "earmarks" were available it would do.

Not only could specimens that appeared to be too old undermine the belief in "eoliths", but specimens that seemed too recent as well. The fact that assemblages of "Eolithic" type had been found in geological strata that elsewhere had yielded Paleolithic or even Neolithic tools seemed like conclusive evidence against human manufacture to the skeptics. To Obermaier these assemblages implied a strange and momentary cultural "collapse" among the group or groups who were supposed to have fashioned them, an abrupt change that made an odd contrast with the lack of evolutionary change in the "eoliths" themselves. To MacCurdy the uniformity of "old" and "new" told in favor of the "eoliths", for it seemed to illustrate the quite logical idea that "a majority of real eoliths are improvisations, and improvisations of one epoch are very like those of another."[196]

The way in which MacCurdy, the believer, and Obermaier, the doubter, came to opposite conclusions about the same pieces of evidence points up an important fact about the debate over "eoliths" -- that to a large degree the differences between the two sides were matters of temperament or perspective rather than decidable propositions. Obermaier did not consider the belief in an "Eolithic" stage of culture an illogical idea, for, he said, "the well-developed industries of the Quaternary would logically lead one to infer preceding stages in which the shaping of stones was effected simply by means of chipping away or retouching."[197] This was a statement with which MacCurdy could heartily concur, but to establish the reality of such a stage Obermaier required strict proof -- the elimination of natural causes by clear association of "eoliths" with undoubted evidence of human presence, either through skeletal remains, or "kitchen refuse."

For MacCurdy, given the plausibility of the "Eolithic" stage, the burden of proof seemed to be on the opponents of currently claimed specimens. Thus, he asserted that "the difficulty of drawing a hard and fast line of demarcation between the artificial and natural cannot be regarded as either proof or disproof of the existence of man-used eoliths. The probabilities are in favor of them; the evidence against them is largely negative."[198] To make a plausible case for natural action did not disprove the existence of "eoliths" but merely showed that their authenticity was not yet demonstrated. In the meantime they deserved the benefit of the doubt.

Given the low requirements of proof that he demanded for it and his refusal to think statistically rather than "typologically" about it, the long durability of the "eolithic" theory and of the evidence underlying it in MacCurdy's work is not surprising. It is an interesting fact, though, that in Human Origins he gave it featured treatment for the last time. To be sure, in the popular article he wrote in 1926 on the three stage theory of cultural evolution MacCurdy restated his belief in an "Eolithic" stage of culture, but he did not bother to review his evidence.[199] In the late 1920s while Osborn was using the "eoliths" of Reid Moir to support his new and extreme version of polyphyletism, MacCurdy wrote only about Paleolithic discoveries in his own articles on prehistory.[200] In his last summary of prehistory, the 1931 volume The Coming of Man, MacCurdy made only a passing reference to "eoliths" -- a cautionary statement to the effect that "Eolithic is the name that should be reserved for artifacts that can be referred definitely to the Tertiary epoch."[194] That he did not go over the ground again might signify merely lack of interest in a problem he had already "taken care of," but it could also point to a muted, but significant retreat from a weak position. Which alternative was involved was not clear from the published record.

What common pattern emerges from the preceding analysis of the major themes in MacCurdy's writings? MacCurdy's status as a patient, conservative synthesizer rather than an innovator comes through most clearly. In his case a kind of genial "old guardism" was involved that contrasts with one's normal image of an intellectual conservative. MacCurdy did not fight change in the discipline of paleoanthropology. Indeed, by sponsoring the research at Mt. Carmel and by helping to organize the 1937 conference on Early Man in Philadelphia, which informed American scientists about further finds of Java Man and the South African australopithecines, he can be said to have contributed a great deal to changing traditional concepts of hominid evolution. Rather, MacCurdy evinced an intellectual style that could incorporate change only with difficulty. He came to the issues that he defined as critical for the study of prehistory relatively early in his career, and stayed with them, without altering his views a great deal; this was especialy true when his ideas coincided with those of the experts on that issue whose opinions he valued most.

This absorption in, and consistency of opinion about a limited number of "key" issues was clearly associated with another central characteristic of MacCurdy's thought. He allowed his preoccupation with a range of discrete, middle-range "problems" -- the Neanderthal - Upper Paleolithic succession, the definition of culture sequences, the psychological function of Paleolithic art, -- to dominate the allocation of his scientific energies to the exclusion of broad theoretical concerns. The three stage theory of cultural evolution that he enunciated in the 1920s was the closest he came to such theories. While the preceding discussion has made it clear that the way he approached this issue was of a piece intellectually with his other work, it never really occupied a major place in MacCurdy's professional priorities. This shows in several ways -- in the ease with which he could leave his first stage, the "Eolithic," out of account in his late writings, in the fact that the clearest statements of the three stage theory came in writings addressed to a general audience, and in the fact that the theory never appeared to guide his research efforts.

Lacking a general theory of how humans evolved made for a great degree of flexibility. One could compartmentalize one's efforts, and deal with or drop important "problems" with relative ease. But without a theoretical framework it was difficult to correlate the pieces of evidence that emerged from consideration of one's "problems." A writer like MacCurdy seemed able only to fall back on an anecdotal style when he chose to describe the process of human emergence. This difficulty showed through clearly in both Human Origins and The Coming of Man -- in order to provide a general perspective on the process of cultural evolution MacCurdy set aside several chapters to discuss the growth of important trait complexes, but in each instance his explanation of change relied on purely psychological and non-evolutionary terminology and concepts.[202]

This sort of divided scientific consciousness, where theory is only presented as semi-popular speculation, and the accepted research problems of the discipline are treated in relative isolation from each other, may be characteristic of a certain type of mind. Osborn, after all, was able to avoid it. But perhaps it was in part the price one paid for a thorough knowledge of the field as it then existed. Osborn was able to reach his unifying theories only at the cost of major oversimplifications. That MacCurdy, whose specialized knowledge of and care with the details of prehistory was so much greater, was unable to propound unifying, and testable, theories leads one to suspect that the state of the discipline reinforced whatever degree of tunnel vision existed in the individual.

Not only, as we have seen, were there major problems barring coherent theorizing in the extant body of paleoanthropological evidence in MacCurdy's day; also the fact that MacCurdy was the first American to bring Old World prehistory squarely into the academic mainstream might have put pressure on him to exercise the prudence, caution and conservatism that have been marks of respectability in science as in other human enterprises. That both personal and extrapersonal factors were involved is highly probable; the relative potency of extrapersonal factors can be tested, in fact, by an examination of a contemporary American scientist whose knowledge of the data on fossil hominids was as extensive as MacCurdy's was on the archeological record. The logical choice for such a test would be Aleš Hrdlička, whose prestige in the field of physical anthropology was equal to that which MacCurdy had attained in prehistoric archeology. It is thus to Hrdlička that we will turn next.

CHAPTER III

ALEŠ HRDLIČKA, 1869-1943

Hrdlička's Background and Early Career

Aleš Hrdlička, longtime Curator of Physical Anthropology at the U.S. National Museum in Washington, D.C. has a solid claim on the title of founder of the discipline in this country. He was the first full-time worker in the field with official museum sponsorship, and this at a time when universities were only beginning to establish general anthropology in their curricula. Hrdlička was also the founder and first editor of the American Journal of Physical Anthropology, the most prestigious and in its early years the only journal devoted to the subject in America.

An indefatigable and careful researcher, Hrdlička produced an impressive list of articles and monographs, on subjects as diverse as the anthropometry of "old stock" Americans, tooth form and dimensions in higher primates, and the first appearance of humans in the Western Hemisphere. Hrdlička also wrote extensively, and at various stages in his long career, about fossil hominids and human evolution generally. It is the works dealing with these subjects that will be the main focus of discussion here; it is important to remember, however, that the theme of human evolution ran as a leitmotif throughout Hrdlička's writings, and the ideas about the process that he developed in essays on other subjects are important to an understanding of his work in paleoanthropology.

Alone among the writers under study here, Hrdlička was not a "native born" American of British descent, but rather a Czechoslovakian immigrant. He was born in 1869 in the town of Humpolec, about sixty-five miles southeast of Prague. The region around Humpolec had long been a center for the production of textiles, and Hrdlička's paternal grandfather had earned his living as an independent weaver. In the latter half of the 19th century, however, mechanization was rapidly transforming the industry; it was probably recognition of this trend that had caused Hrdlička's father, Maximilian, to be apprenticed to a cabinet maker who also built power looms for local mills. After completing his apprenticeship, Maximilian Hrdlička succeeded to the management of his own cabinetmaking shop, and apparently was in good enough economic circumstances to choose an educational path for his firstborn Aleš that would have led to the university had the family remained in Czechoslovakia.[1]

Though the Hrdlička family clung to middle class status during the 1870s, business conditions had not been good for Maximilian. Like so many others of his time, the elder Hrdlička came to believe that his talents and hard work would be better rewarded in America. When he emigrated in the fall of 1881 he brought Aleš along. The pair settled in New York City, and to help raise the funds required to bring over the rest of the family the young Hrdlička worked for a time in one of the metropolis' numerous cigar factories. Once the family was settled, however, Aleš was apparently able to resume his schooling, for in 1889 he enrolled in medical school, specifically the Eclectic Medical College of the City of New York.[2]

After graduating first in his class in 1892, Hrdlička set up practice on East 57th Street in Manhattan, which was then a German working class district. He apparently did not, however, even at this stage of his life, really wish to follow the career of a general medical practitioner, but instead undertook further medical studies at the New York Homeopathic College. Upon graduation from that institution in 1894, again first in his class, Hrdlička accepted an offer to become a staff physician at New York's Middletown State Homeopathic Hospital for the Insane. Hrdlička's path to physical anthropology began at Middletown; it was there that he did his first research on a topic that was clearly related to the discipline -- a study of the possible association between various forms of insanity and "physical type."[3]

The experience that really solidified his commitment to physical anthropology, however, was the work he did at the Ecole d'Anthropologie in Paris in 1896. Hrdlička had been drawn to Paris because of the high quality of medical instruction available there; such pilgrimages were by no means uncommon for American medical school graduates of that period, given the generally recognized inferiority of U.S. schools. The Ecole d'Anthropologie enjoyed an especially high reputation, for it had been founded by the renowned anthropologist Paul Broca (1924-1880), and through his efforts had become the center of anthropological research in France. At the Ecole Hrdlička's most influential teacher (and later close friend) was Leonce Manouvrier (1850-1927), himself a major figure in the history of French anthropology.[4]

After his return from France, Hrdlička was able to undertake his first extensive work as a physical anthropologist as an employee of the Pathological Institute of the New York Hospitals. The Institute had been set up to pursue a broad range of studies on the organic causes of insanity; Hrdlička's responsibility was to collect and analyze data at the "macroscopic" level to determine whether the insane differed anatomically or physiologically from normals. Professional and personal friction within the institute eventually made Hrdlička's position difficult. A bridge to a more satisfactory position was provided by the association that Hrdlička developed during his stay at the Institute with Frederic Ward Putnam (1939-1915), who was then in charge of the anthropological division of the American Museum of Natural History. Through Putnam, he got his first field experience among the native populations of North America. The anthropometric studies Hrdlička conducted as a field anthropologist for the American Museum in the years from 1899 - 1903 among the Indians of the Southwest and northern Mexico were, as Hrdlička's biographer Frank Spencer notes, "the most extensive somatological investigation undertaken by a single worker in the U.S." up until that time.[5]

Hrdlička's years at the American Museum also provided him with his introduction to a major controversy relating to early man, one in which he was destined to become a major figure. The issue was human antiquity in North America -- Putnam was inclined to accept the high antiquity of human beings in this hemisphere; his principal opponent, William Henry Holmes of the Bureau of American Ethnology at the Smithsonian, argued that the discovery of the primitive-looking "Pithecanthropus" by

Eugene DuBois confirmed the views of those who had never believed that Homo sapiens had been present in the Americas during glacial or pre-glacial times. Putnam asked Hrdlička to look at two samples of skeletal material that had been cited as evidence of "glacial man," one from New Jersey, and the other from Kansas. Concerning the first sample Hrdlička reserved judgement, but on the second he concluded against great antiquity, arguing that the skeleton of the so-called "Lansing man" was clearly that of a recent Plains Indian.[6]

Hrdlička had made a vigorous, and promising, beginning as a physical anthropologist at the American Museum, but his future in the discipline came into question when the museum's head administrator, Herman C. Bumpus, began to reduce the institution's commitment to anthropological research. Putnam left to take up a post in California, and Hrdlička was faced with the prospect of imminent unemployment. Fortunately, Holmes, who had been much impressed by Hrdlička, was seeking to create a Division of Physical Anthropology at the Smithsonian's National Museum; when that division was established in 1903, Holmes was able to secure the appoinment as curator for Hrdlička, a position which the latter was to hold for the remainder of his long career.[7]

1907-1915: Development of Fundamental Themes

Though Hrdlička did not publish anything on the stages of human evolution prior to the appearance of *Homo sapiens* until 1913, several of the key ideas that emerged in his work between 1907 and 1912 are, as Spencer has pointed out, central to an understanding of his later writings on the subject. His continuing investigation of the issue of the first appearance of human beings in the Americas is especially important, for the conclusions, both theoretical and substantive, he reached on that subject are logically interrelated with those he would come to make regarding hominid evolution in the Old World.

Hrdlička's comprehensive analysis of all the major finds of allegedly ancient human remains in North America predated the one he did on South America by several years; however, in both cases the overall judgement was basically the same -- none of the finds could be accepted as evidence that human beings had inhabited any part of the Western Hemisphere earlier than the beginning of the post-glacial era. Hrdlička was able to point to numerous instances where evidence of intrusive burials had gone unnoticed, geological deposits had been incorrectly dated, and artificial skeletal deformation or the effects of disease had been taken for "primitive" morphological features. Because of his wide acquaintance with skeletal variation in the American Indian population, Hrdlička was also able to match allegedly "primitive" features in the "fossil" material under analysis with specimens from undoubtedly recent Indian burials.[8]

The practice last mentioned has led some of Hrdlička's critics over the years to charge him with the error of "morphological dating" -- i.e. determining the geological age of a specimen primarily on the basis of its morphology rather than on the characteristics of the stratum in which it is found, but the charge is not quite accurate.[9] In his own summary of the dating criteria he was using Hrdlička pointed out that[10]

> identification of human bones as those of early men -- that is, man of geological antiquity -- demands indisputable stratigraphical evidence, some degree of fossilization of the bones, and marked serial somatological distinctions in the more osseous parts. A skeleton or a skull not fossilized or one (whether fossilized or not) agreeing in most of the more essential features with the skeleton or skull of a recent, or not very ancient, man in the same locality, can not be accepted as geologically ancient, unless the geological evidence should be absolutely decisive.

Conversely, he noted, specimens with "features characteristic of inferior stages of human development" did not have an automatic claim on geological antiquity, but ought to be supported by other lines of evidence.[11]

Though this formulation showed the critical importance that Hrdlička attached to geological proofs of antiquity, it also seemed to create a sort of "double standard" in his procedure that would distinguish him from those who were to be more sympathetic to the idea of early Pleistocene Homo sapiens in the years after World War I. It was the phrase "marked serial somatological distinctions" that created the problem. Though not crystal clear, it seemed to be a restatement of Hrdlička's view, expressed a bit earlier in the same essay, that "somatologically, the bones, and particularly the skull, of early man may be confidently expected to show some differences from those of modern man, especially in the direction of lesser differentiation."

In the abstract this was a reasonable hypothesis, but it assumed that gradual, progressive change in human morphology was the normal order of things. Indeed, Hrdlička made this assumption quite explicit when he asserted that[12]

> heredity ... especially in so far as it applies to the latest acquired characteristics of the skeleton, is subject to incidental irregularities as well as to gradual modififcations. Habits of muscle action, on the other hand, change with environment and culture; such changes in activities may take place much more slowly in some localities than in others, yet they are bound to manifest themselves everywhere in the course of ages and to be followed by corresponding and recurring structural alterations.

These skeletal changes might not all be of great importance, and detailed study had not been done on all of them as yet, but Hrdlička was convinced that both the skeltal diversity of recent populations and what was known about the "geologically ancient crania of Europe" made the theory of long continued, and continuing, gradual change a sound one.[13]

If all this were true, how should one deal with an allegedly ancient specimen that did not differ much from present human form? For Hrdlička, the geological proof of antiquity would obviously have to be, in his words, of an "absolutely decisive" nature. In theory, that does not sound like a harsher standard than the "indisputable stratigraphic evidence" that he demanded of all candidates for inclusion in the catalogue of human fossils, but in practice it would be difficult to avoid making it harsher, given his theoretical commitment to the hypothesis of gradual change. In his handling of what were for him the troubling Piltdown remains,[14] Hrdlička did indeed provide grounds upon which his critics could have accused him of straining the evidence in order to avoid unwelcome conclusions.

Just as the theoretical views laid out in Hrdlička's early works on the prehistory of the Americas help illuminate his later writings, so also do the substantive conclusions therein. The absence of any well-dated representatives of Pleistocene humanity in the New World, and especially the removal of so many pretenders to that status, made Hrdlička feel nearly certain that human beings had not reached this continent until the very end of the Ice Age, or thereafter. Also, if people in a high stage of Paleolithic culture had been the first inhabitants, they must not have reached the regions adjacent to the most likely migration route, the Bering Strait, until relatively recently as well; if they had been in northern and eastern Asia earlier, he reasoned, they would have penetrated into North America in more ancient times too. Thus, presumed migration patterns, as well as the locations in which the great preponderance of Pleistocene fossil hominids had been discovered up to that time, seemed to make a Europe-centered picture of human evolution a reasonable one for Hrdlička.[15]

The first attempt to provide such a picture was not long in coming, for his studies on the New World had apparently convinced Hrdlička that the whole body of evidence on the emergence of _Homo sapiens_ was in need of comprehensive analysis. In the spring and summer of 1912 he undertook a personal examination of "all the more important skeletal remains ... preserved in the museums of

Europe;" his descriptive, and interpretive, catalogue of those remains was duly published in the Smithsonian's Annual Report for the year 1913.[16]

Nearly all of Hrdlička's discussion was devoted to individual specimens, but he did try at the start to characterize his general conception of the course that human evolution had followed. In short, he saw the process as having been a regular, though not "uniformly accelerated" one, with the critical changes over time being those of "reduced teeth, larger brain, [and] more erect posture, with increased facility of intercommunication." He also made plain his belief that those changes had taken place "under the influence, in all probability of changing environment, more especially food and climate."[17] Because the process had not moved at a uniform rate in all populations at all times, the "more immediate" human precursors displayed "various individual advances" in a modern direction. In fact, Hrdlička allowed for the possibility that, even after the line separating the human from the prehuman had been passed, several variant strains had existed, some possibly becoming extinct, while "others kept on modifying in the upward direction until in the course of long ages they reached the various somewhat unequally advanced types of man of the present day."[18]

This general description seemed to leave open the chance Hrdlička might have accepted the idea which Hooton would later advance (and Keith was already developing) -- i.e. that human evolution had proceeded along a "multilinear" and "discordant" or "asymmetrical" course. Still, even at this early stage in the development of Hrdlicka's theories on fossil humans, it was evident that he was going in a different direction from the majority in England and the U.S. He did not, specifically, opt for the early Pleistocene appearance of Homo sapiens, the cornerstone of the Keith-Hooton position. In the more general passages of his monograph Hrdlička left this option open, since he gave "the end of the Tertiary" as the period in which there existed creatures in the final stages of transition from the prehuman to the human level, i.e. creatures "approaching present man in size of skull and brain, in the character of the teeth, in stature, in the form of the pelvis, and in other particulars ...[perhaps even the] use of articulate language."[19] In the way he characterized the various fossils he saw as relevant to the question of the emergence of Homo sapiens, however, he pretty much closed the door on the early sapiens alternative.

On the most important of these fossils Hrdlička's judgements were as follows. Regarding the morphologically most primitive specimen that had been discovered prior to 1912, "Pithecanthropus erectus," he felt he had to hedge a bit, because of the inaccessability of the originals for examination, a situation that he considered lamentable.[20] Still the impression that he had derived from the casts of and the literature on "Pithecanthropus" was that this "hitherto unknown primate form ... whether or not man's direct ancestor, stands morphologically between man and the known anthropoid apes, and fills an important space in the hitherto existing large void between the two."[21]

About the mandible which had received the name "Homo heidelbergensis" he was willing to be more definite. Though many of the characters of the jaw and chin were "exceedingly primitive," the form of the teeth was in his view "unquestionably human," despite their large size, "great roots" and other primitive features. On the basis of this combination of characters Hrdlička ventured a reconstruction of the rest of Heidelberg man -- he hypothesized a creature which

> while of heavy, protruding face, huge muscles of mastication, wide and thick zygomatic arches, thick skull, probably heavy brows, and possible not yet quite erect posture, had nevertheless already stepped over that line above which the being could be termed human. His food and probably his mode of life were related to those of primitive man, and he was already far removed from his primate ancestors with large canines.[22]

Like the early MacCurdy and the early Gregory, then, Hrdlička portrayed both Java man and Heidelberg man as progressively more modern morphological intermediates between anthropoids and humans; he differed from the new consensus that was developing because he did not try in any way to exclude these fossils from possible direct ancestry to more modern forms of man. While it seemed equally easy for Hrdlička to hold open a place for both of these creatures in the human family tree, there was actually a problem implicit in the inclusion of "Pithecanthropus" -- namely, if Asia had been the home of an early human ancestor, why should it not have contained the latter's descendants as they evolved into anatomically modern Homo sapiens ? As Hrdlička developed his theory about the peopling of the earth in more detail during the 1920s, he took note of, and tried to overcome this difficulty. Apparently, however, in 1913 his ideas had not yet fully crystallized.[23]

Regarding the fossils of the Middle and Upper Paleolithic epochs, Hrdlička's opinions were much more fully formed by 1913. In fact, one could say that the basis for his famous argument in favor of the "Neanderthal phase of man" was already present in the statements he made in 1913 regarding specific fossil populations from Europe representing these eras. Thus, at one point, in discussing the "type specimen" -- the Neanderthal skeleton itself -- he portrayed it as representing a clear transitional form, a "human being already far advanced above any anthropoid," but still on a "lower scale of evolution than any man of today."[24]

In his description of the large population unearthed at Krapina, Yugoslavia, Hrdlička took care to draw out the implications of the fact that these remains, like others from the "Mousterian epoch," were characterized by relative "lowness of the [skull] vault, and in every instance among the adults by a pronounced, complete supraorbital arc." In his view this constituted "definite proof of the fact, not quite well established before, that this arc was up to a certain phase of the Quaternary period a regular characteristic of the early man of a large part of Europe."[25] In addition, the jaws and teeth of the Krapina population, while more robust and primitive looking than those of later humans, were, he said, intermediate between the latter and the Heidelberg jaw. He concluded on the basis of such observations both that the Krapina people were "a group belonging to the family of <u>Homo neanderthalensis,"</u> and that this group was not an extinct, collateral branch of the genus <u>Homo,</u> but "more probably a direct and not excessively far distant ancestor of <u>Homo sapiens.</u>"[26]

In its fully developed form, Hrdlička's "Neanderthal phase" thesis would rest, in part, upon the contention that Middle and Upper Paleolithic populations showed significant approaches to each other in morphology, particularly of the skull, and this theme was represented in the 1913 essay as well. In the Spy crania from Belgium, which Hrdlicka felt had been justly classified as Neanderthals, he noticed a "trace" of the modern human chin prominence present in the skull known as Spy I. The Spy II skull, though possesssd of the typical Neanderthal "supra-orbital arch," had, he asserted, "a considerably higher and more convex forehead;" in fact, he noted that the "whole vault" was "higher and more spacious," a form approaching "in many respects that in modern man."[27] For these reasons, he believed, they were close to being "transitional forms" between the Neanderthals and Upper Paleolithic specimens such as the Combe Capelle skull.[28] In his analysis of Upper Paleolithic remains Hrdlička argued that the population found at Predmost in Czechoslovakia, which though not yet fully described he had examined twice, represented "in a measure the much searched for bridge between the Neanderthal and recent man." This being the case, it was not surprising that Hrdlička regarded these fossils from the land of his birth as "the most important assemblage of material from the transitional period between the earlier and the latest paleolithic forms" of humanity so far discovered.[29]

An intriguing, and perhaps puzzling, feature of Hrdlička's handling of the Neanderthal issue was his general acceptance of Marcellin Boule's evaluation of the La Chapelle-aux-Saints fossil. To be sure, Hrdlička highlighted the more advanced aspects of the skull, such as the fact that its forehead "while low" was "somewhat better formed" than in the Neanderthal and Spy I skulls. He also pointed out that the circumstances in which the skeleton was found represented "plainly a regular burial, the most ancient intentional burial thus far discovered."[30] Still, he did not question any of Boule's identifications of primitive features in the La Chapelle fossil's skull and limb bones. In addition, he concurred in Boule's judgements on the endocranial cast. Thus, the large size of the fossil's endocranial cavity did not indicate to him "necessarily a superior brain, but rather one subserving to largely developed organs and powerful musculature." In the cast itself, there were "more strictly human features" such as the "predominance in size of the left over the right hemisphere," but also "more simian" ones like "the evident simplicity and coarseness of the convolutions, and the relatively poor development of the frontal parts."[31]

Hrdlička probably did not feel the need to criticize Boule's findings, because the mixture of human with "more simian" features was exactly what the former expected in a "transitional" hominid form, even one of the relatively recent past. But as we have seen elsewhere, for Boule and for most of those who agreed with his characterization of the Neanderthals, it was exactly these supposed "simian" features that made the Neanderthals unlikely human ancestors. Thus, it surely would have served Hrdlička's position had he looked into Boule's reasoning and evidence more closely.[32]

The discovery that would do the most to undermine Hrdlička's characterization of the course of human evolution was, however, not the La Chapelle-aux-Saints skeleton but rather the remains of Piltdown man. These had appeared too recently for him to have examined them personally during his stay in Europe, so he had to confine himself to reporting the preliminary results which had been put forward by British scientists. Rather than commit himself to a specific interpretation, he would only conclude that Piltdown represented "doubtless one of the most interesting finds relating to man's antiquity, though seemingly the last word has not been said as to its date and especially as to the physical characteristics of the being it stands for."[33] The "last word" would not be said until the 1950's but in his later writings on fossil man Hrdlička would be forced to develop his own hypotheses about both the age and the "physical characteristics" of the Piltdown material; being noncommittal about Piltdown man was a position Hrdlička could not afford to adopt, and in the early 1920s he would work hard to harmonize the Piltdown discoveries with his general views.

Consolidating a Theory on the "Peopling of the Earth"

In the half decade that followed the publication of "The Most Ancient Skeletal Remains of Man" Hrdlička did not undertake further significant contributions to the debate on hominid phylogeny. These years were still important, though, for they saw him make advances in a line of study that was closely related -- the functional analysis of tooth form in modern human populations. The reasons why Hrdlička would concern himself with the study of the dentition have been made plain by Spencer. First, as we have already seen, Hrdlička held the strong belief that the major features of human cranial morphology had their origin in functional adaptation to environmental forces, and that food, climate and human culture itself were among the most important of those forces. If this were true, the teeth should have provided clear evidence of these forces at work, since no part of the skull was more directly affected by interaction with, and the interactions among, all three. In fact, Hrdlička gave an early indication of the importance that he assigned to the influence of diet in the following passage about differences between human and orang skull form:[34]

> The study of orang crania as a whole impresses me with the high degree of individual variation and with the role played by the muscles and teeth in modifying various parts. As both these agencies are mainly connected with the kind of food, the plausible suggestion forces itself upon the mind that a prolonged change, lasting through a number of generations, to food requiring less mastication would greatly modify the whole orang skull. It should also bring it nearer to the human type, for the features by which the orang cranium differs from the human are with few exceptions exactly those produced by greater teeth and muscles of mastication.

Hrdlička returned to, and stated even more explicitly what Spencer has called his "dietary hypothesis" in an address before a group of dental professionals in 1911. "If any differentiation in the teeth has taken place among the anthropogenic primates and the earliest representatives of man," he asserted, "these must have been changes in function relating to the teeth ... It is only modification of its function, of its uses, that can modify a passive tool-organ such as the tooth."[35] In the same address Hrdlička noted that the general trend toward reduction in the size and robustness of the dentition from the lower Pleistocene Heidelberg man to modern primitive tribes, a trend in which Neanderthal man could be seen to occupy an intermediate position, could also be explained by the hypothesis; advances in the technology of food preparation and changes in diet had probably made the sturdy dental apparatus of early hominids less and less necessary over time. He also cited variations among recent populations of Homo sapiens that he believed were related to dietary differences.[36]

While he could outline his "dietary hypothesis" in a general way in 1911, Hrdlička felt that the detailed base of evidence which could establish it was still lacking. The first steps that he took in providing that base came in 1915, with an intensive analysis of a morphological feature that he had noticed in his earlier studies on American Indian crania, a feature which he called the "shovel-shaped" incisor. Beginning with an analysis of Eskimo and American Indian crania, Hrdlička also planned a 1916 trip to northern China and Mongolia, to see whether the populations which he believed to be most closely related to those of the New World possessed the character in similar form and frequencies. Because of political unrest in China and the U.S. involvement in World War I that came soon afterward, that trip had to be postponed until 1919.[37]

When the final version of the study on "shovel-shaped" incisors appeared in 1920, it included data not only from Asians and American Indians, but also from samples of American Whites and Negroes. The character, Hrdlička found, had its highest frequencies among the "yellow brown" peoples; though it was considerably less common among both Whites and Negroes, the Negro percentage of 12 was considerably higher than the Whites' 8. Hrdlička also found "shoveling" to be common in the samples of fossil hominids and present-day anthropoids that he had examined for purposes of comparison.

To him the pattern presented by the data confirmed the hypothesis that the "shovel-shaped" incisor was a characteristic with important functional and phylogenetic significance. Specifically, he argued that it was an adaptive response to a "call for strengthening" of the anterior teeth, since a tooth of this shape was "on mechanical principles" stronger than a flat surfaced tooth. If this were so, then the incidence of shoveling should have fallen during the course of human evolution, for as improvements in food preparation and tool technology had taken place, the need for this type of tooth would have decreased.[38] If one looked at the character in present human populations, then (as Spencer has characterized Hrdlička's reasoning) one would expect "to find lower frequencies of shoveling among the descendants of those people who had solved, by cultural means, the problems that shovel-shaped incisors had solved biologically, and who had done so for the longest period of time." Conversely, the frequency would be highest among those groups who had remained "committed to an Upper Paleolithic way of life" -- the most primitive level of culture known in anatomically modern Homo sapiens -- for the longest time.[39]

The high, and comparable incidence of "shovel-shaped" teeth in both Asians and Native Americans could thus support two generalizations that Hrdlička considered important ones. The first was the idea of close genetic relationship between the two groups that had often been hypothesized for other reasons as well. The second was the notion that these groups had, in comparison to the other major racial groups under study, only arrived relatively recently in the regions where they now resided. All that one needed to render the latter idea plausible was the reasonable assumption that populations which had been engaged in a long, and slow, migration from a far distant "cradle of mankind" had retained the nomadic Upper Paleolithic way of life (along with the associated incisors) while they moved.

Interestingly, Hrdlička only stated the first of this pair of conclusions in his essay. However, that the second was in his mind as well is evident, for in the period just after this he began to state his theories about the center of origin and pattern of dispersal of Homo sapiens in fully developed form, and these theories dovetailed perfectly with the implications of the work on "shovel-shaped" incisors.[40] The centerpiece of these theories was the hypothesis that modern humans had appeared first in, or at least very close to Europe. The earlier writings on fossil hominids discussed above had been consistent with this hypothesis, but until 1921 Hrdlička had not attempted to show why Europe was the overwhelmingly probable choice. The context in which he stated his argument for a European center of evolution was a discussion of the "peopling of Asia," which for two strategic reasons was an excellent place to begin -- first, because a late appearance of Homo sapiens in Asia would make his views on the peopling of the Americas more credible, and second, because writers like Boule and Osborn (for somewhat different reasons), were looking eastward to Asia for the birthplace of modern humans.[41]

Since the major part of the Asian population was of the so-called Mongoloid race, the focal point of Hrdlička's account was the latter group's place of origin and its time of arrival on the Asian continent. Also, he asserted, the history of the first appearance of the Mongoloid race would resolve the question of the first appearance on Homo sapiens itself in most of the continent, since no evidence had yet turned up that any other race had been present prior to the Mongoloids. Hrdlička's attempt to identify the area from which the Mongoloid race had come to Asia was of course predicated on his excluding the obvious alternative -- that this group had evolved into Homo sapiens from a more primitive form right on the spot. This option did not have to be considered, he argued, because the lack of prehuman fossils in the "central or northwestern parts of the continent" indicated that there had probably been nothing in the bulk of Asia "from which man could evolve."[42]

While in the years prior to the discovery of Peking man this appeal to an absence of evidence could hold up, however shakily, a fossil hominid had been discovered in southeast Asia, namely "Pithecanthropus," so Hrdlička had to proceed differently in regard to this part of the continent. First, he asserted, expeditions to that region since the discovery of "Pithecanthropus" had revealed neither precursors of that creature nor forms intermediate between it and <u>Homo sapiens.</u> This fact , Hrdlička speculated, might indicate either that some environmental cause or causes had forced Java man to migrate westward, where he evolved toward <u>Homo sapiens,</u> or that conditions for evolution beyond the stage of which "Pithecanthropus" was a representative had been better met in another region, such as Africa; this next stage could then have easily migrated into Europe to produce the Neanderthals and later populations, while Java man became extinct.[43]

The preceding argument reveals that Hrdlička was not thoroughly wedded to a unilinear evolutionary scenario, but that was not the only basis on which he felt himself able to reject the possibility of <u>in situ</u> evolution of the Mongoloid race in southeast Asia. The present day racial make-up of the region, he contended, counted against it as well. Of the two major groups there at present, one group, the Malays, represented an offshoot of the Mongoloids and seemed to be a relatively recent import into the region; the other group, the Negritos, he considered a "weak race physically as well as mentally," whose very presence showed that it must at one time have "occupied these regions unopposed," for it could not "have prevailed over and penetrated through any stronger people."[44] The idea that the Negritos themselves could have been an autochthonous group he also rejected, for he could find no evidence, despite their inferiority, to indicate that the Negritos were a "geologically ancient type."[45]

If the great bulk of the Asian continent had been peopled by immigrants, where could they have come from? The far north and northeast seemed unlikely, because of the harshness of the climate, especially during the critical period of the late Pleistocene. The southwest was possible, but the apparent absence of Mongoloid blood in the peoples of "hither India" argued against it -- if the ancestors of the Mongoloids had been in the latter area for any length of time, they would have mixed with the groups that had moved in either before or after them. Since the southeast was also unlikely -- there seemed to be only one plausible migration route: i.e. the route "from the west through the great flat lands to the north of the Himalayan and central Asiatic mountains." This route, of course, would connect the ancestors of the present day Mongoloid race with the "old European peoples;" the time of the migration from Europe, he thought, judging from the "main physical traits" common to both groups, had probably not been until the "late Paleolithic and succeeding periods."[46]

While being able to give a European place of origin for the vast bulk of the Asian population was a feat of considerable ingenuity, there was still an anomalous group near Asia whose peculiarities Hrdlička felt he had to account for. This group was the aboriginal population of Australia, a race which was widely conceded at the time to be among the most primitive forms of Homo sapiens, ancient or modern. The existence of such a group, similar in morphology, and perhaps even in culture, to the Upper Paleolithic peoples of Europe seemed to give Australia and/or the parts of Asia easily accessible to it an excellent claim as a zone of human emergence in its own right. Hrdlička, however, drew nearly the opposite conclusion from the similarity of the Australoid race to the European Upper Paleolithic peoples. "That such similarities could have developed independently in two environmentally widely different regions," was, he contended, "to say the least, very improbable." And if there had to be a single place of origin, Europe was the most likely candidate.[47]

Hrdlička was aware that the picture of a late appearance of humanity in a single center, a picture which necessitated numerous migrations over long distances in the not too distant past, might be difficult for some of his readers to accept. However, if one thought about it, he argued, this picture seemed to make sense on the grounds of general theoretical principles as well as on its ability to account for existing evidence. If human evolutionary success had been first and foremost a product of human mental and cultural capabilities, then, he said, one ought to expect hominids to have become a widespread group on the earth only when these capacities had reached a high order of complexity. Since this behavioral sophistication had come late in human evolution, so also had the geographical disperal of the various forebears of present day races.[48]

The argument on the whole seemed to commit Hrdlička to a picture in which waves of Homo sapiens radiated out of Europe, in a manner similar to the one in which Osborn conceived of them radiating out of central Asia. Hrdlička of course believed that his choice for the actual center of human dispersal was the right one; the character of his argument, however, which relied so heavily on the fact that human skeletal and cultural remains had not been found in certain areas, made it unlikely that he would convince those who believed that these areas had yet to be fully explored. It is no surprise then, that writers like Osborn and Davidson Black continued to question the primacy of Europe in human evolution. Hrdlička returned to the issue in 1926, the heyday of Osborn's central Asian idea, this time in an article about the peopling of the entire earth, and not merely the continent of Asia. While the scope was broader, the thesis was the same -- i.e. that Europe was the region in which "the first beings that could be called human came into existence."[49]

Predictably, a critique of Osborn's competing theory was an essential part of Hrdlička's article. In short, Hrdlička's judgement was that Osborn's hypothesis, "the idea that the cradle of man lay in central Asia, may be characterized as merely an idea, based on collateral rather than critical anthropological reasons and without to this moment a single item of material evidence." All of the human remains found in Asia up to that point had come from post glacial deposits; since, as he had long argued, "the infancy of the human race" belonged "to the earlier half of the glacial period," and "not a vestige of substantial evidence" from this time span existed as yet outside Europe, Europe's claim as the center of human origin was still secure.[50] What Hrdlička meant exactly by the "earlier half" of the Pleistocene he did not define, but apparently it did not include the period in which "Pithecanthropus erectus" had lived in Java.

Another interesting aspect of the passage was the almost scornful tone he employed in referring to Osborn's theoretical arguments. The latter's attempt to place human evolution in a general mammalian context apparently seemed suspect to Hrdlička because it did not proceed directly out of specifically "anthropologcal" evidence. The role of general evolutionary theory as a guide in the search for evidence as well as in its interpretation was apparently one which he did not consciously recognize, yet in his work on function and adaptation in skull form he actually gave it this role in a way not so different from Osborn.

Perhaps the contemptuous tone that Hrdlička projected in this passage had a broader reference than to Osborn's central Asian idea alone. The emergence of the latter scientist in the early 1920s as a spokesman on the racial aspects of "eugenics" during the drive to restrict immigration constituted an even bolder instance of "poaching" on anthropological territiory; as an immigrant from one of the countries discriminated against in the ensuing "national origins" quotas Hrdlička could not have been ignorant of, or pleased with the role Osborn had played. Indeed Hrdlička's competing theory of human emergence, with its geologically recent waves of emigrants spreading from Europe, might have fitted his social position as well as Osborn's notions of parallel phyla and orthogenetic specialization fitted a scion of the "old American stock."

In the course of countering Osborn's theories about human dispersal with his own, Hrdlička previewed what would be a central point of his thesis on the "Neanderthal phase of man" -- i.e. the idea that Neanderthal man constituted a "necessary stage of man's evolution." What he meant by "necessary" was the following: even if it could be demonstrated that the European Neanderthals were not ancestral to modern humans, an ancestral group very much like them would have had to exist elsewhere.[51] While Hrdlicka did, as we shall see in our analysis of his famous address on the "Neanderthal phase," have "critical anthropological evidence" for his views, much of the argument also rested on his theoretical conception of what earlier hominids must have been like before they became fully human.[52] To Hrdlička's critics at the time, it must surely have seemed that these theoretical expectations about what hominids must have been like were causing him to ignore "critical anthropological" facts that seemed just as plain as those he was employing.

Another aspect of Hrdlička's thinking about the Neanderthal problem that also emerged in the 1926 article concerned the supposed "replacement" of the western European Neanderthals by "Aurignacian man" during the last glaciation.[53] While Hrdlička has sometimes been described as subscribing to a simple unilinear view of human evolution,[54] he actually could, as we have seen in regard to "Pithecanthropus erectus," make allowance for the possibility that there had been some extinct side-branches on the human family tree. In fact, he was even willing to consider the theory that the "classic" Neanderthals of western Europe had themselves been an evolutionary dead-end. In "The Peopling of the Earth" he put forward a possible scenario quite similar in outline to the well-known one proposed much later by F. Clark Howell,[55] a scenario that could account for the abrupt transition between the later Neanderthals and early Aurignacian populations, if indeed it were found to be abrupt. His scenario involved the simple supposition that while the "Neanderthal type was declining" in western Europe due to the "vicissitudes" of the last glaciation, "portions of it [the type] which had extended into and possibly beyond central Europe, developed gradually into the Aurignacian man, who spread once more westward, and reoccupied most if not all the sites of his Neanderthal forefather."[56] It was thus possible for some western European Neanderthals to have been dead-ends, and for others to have been the authentic ancestors of *Homo sapiens;* this was an idea to which he would return, though not in exactly the same form.

Molar Teeth and the Piltdown Problem

In order to understand fully the context in which the most important of Hrdlička's later writings on human evolution took shape, one must also take note of another key aspect of his scientific activities in the first half of the 1920s -- the continuation of his detailed study of dental evolution, and in particular the evolution of the molar teeth. As Spencer has shown,[57] this round of investigations was motivated in large part by a desire to resolve the troublesome question of Piltdown man.

Hrdlička had been relcutant to accept the claims that were being made about Piltdown man from the beginning, and as his own theories matured his uneasiness must have increased, for both the date and the morphological characteristics of "Eoanthropus dawsoni" put forward by its defenders called into question key elements in Hrdlička's picture of the origin and spread of humanity. First, as Hooton was to assert vigorously,[58] "Eoanthropus" seemed to give the lie to the theory that the main line of human evolution had had to proceed through heavy-browed, low-vaulted forms like the Neadnerthals or "Pithecanthropus." In addition, the fossil gave support to those who rejected the notion of a Neanderthal stage in favor of an early Pleistocene emergence of Homo sapiens, for while the Piltdown jaw was apelike, the skull vault was difficult to distinguish from modern crania. Also, the strange mixture of simian and hominid characters in Piltdown did violence to the picture of gradual, correlated advances in various aspects of morphology and culture that was a crucial theme in Hrdlička's evolutionary scenario. Finally, if the modern human species had appeared early in the Pleistocene, Hrdlička's arguments on the "peopling" of Asia and America would be in jeopardy as well.

The most obvious way to undermine the various claims that had been based on "Eoanthropus" was to dissociate the skull and jaw by showing that they had belonged to different creatures. When Gerrit S. Miller, Hrdlička's colleague at the Smithsonian, developed an interpretation along these lines, Hrdlička suported it, and gave Miller space in the very first issue of the American Journal of Physical Anthropology so that the latter could respond to his critics.[59] The supporters of "Eoanthropus" launched a strong attack against Miller's ideas in the years after 1915.[60] They tried to point out ways in which the jaw and teeth of Piltdown man did not approximate those of any known chimpanzee (Miller had given the jaw the designation Pan vetus, because it seemed to be geologically ancient and differed in small ways from recent chimpamzees).

Perhaps more important, they attacked the logic of Miller's attribution of the bones to separate species. How likely, they asked, was it that one would find remains of creatures so different in their mode of life as early man and the chimpanzee juxtaposed in a geological deposit that had never before yielded human or anthropoid fossils? Did it not demand too much to ask one to believe in "Pan vetus," when the existing fossil record in all of western Europe for the time period supposedly represented at Piltdown contained no traces of great apes? Finally, did not the announcement of a second find at Piltdown, in which skull fragments were apparently associated with teeth, prove that "Eoanthropus" was only one creature? As Stephen Jay Gould has pointed out in an article on the controversy,[61] it might have been easier to claim that a hoax had been perpetrated than to believe that two authentically ancient species had been discovered.

While in a purely logical sense Gould's observation might be accurate, Hrdlička's approach to the problem was different, and understandably so. Geological probabilities aside, he believed that neither Miller nor the latter's critics had had the last word on the Piltdown jaw and teeth. Further study of the Piltdown material, in the context of a broad sampling of specimens from anthropoids and humans, both recent and fossil, might settle the question. As Hrdlička began to get involved in this study he came to believe that the body of comparative data that he required did not yet exist. Not enough had been done in setting out the limits of normal variation in molar tooth form, and the data that had been compiled was difficult to use because of differences in systems of measurement and confusion in nomenclature.[62] He thus tried to extend the reach of his studies, so that they would not only contribute to the solution of the Piltdown question, but also put the study of the evolution of the human dentition on a firmer basis.

As part of his research effort, Hrdlička not only collected data from collections of recent human and anthropoid teeth in American museums, but also journeyed to Europe to examine original specimens from fossil hominids and dryopithecines. In the summer of 1923 he even had the rare experience of examining the Piltdown specimens as well as those of "Pithecanthropus" first hand.[63] The major general conclusion that Hrdlička advanced on the basis of this new data was that the size of the molars had diminished steadily during the course of hominid evolution, a change which had also brought about progressive "shortening" of the jaw and concurrent changes in the shape of the face. As he had long since been arguing in a more general way, he also found that the Neanderthals fit very neatly as an intermediate stage in this morphological trend.[64]

Hrdlička's results on the Piltdown problem were not so clear cut as his reflections on the general trend of dental evolution. In an article on the Piltdown jaw published in 1922, apparently on the basis of casts and photographs of the specimen (he confirmed his conclusions however in a brief notice written upon returning from Europe),[65] he made clear his abandonment of Miller's "Pan vetus" theory. He still proved reluctant, though, to accept the association of the Piltdown jaw and skull -- the contrast between the "gracility" of the jaw and the thick, robust appearance of the skull bones left him with the feeling that there was "no perceptible correspondence" between the two.[66] Hrdlička clearly realized that the discovery of the so-called "Piltdown II" specimens had made the dis-association of the skull and jaw seem impossible to many observers, but he tried to counter that view by raising the notion that the Piltdown II molar was actually one of the teeth missing from the Piltdown I jaw. Thus there might only have been two skulls and a single jaw represented rather than a pair of each.[67]

What was actually most surprising about Hrdlička's treatment of the Piltdown fossils was not that he tried to keep the skull and jaw separate, but rather that he tried to make both into hominids. Thus, he described the jaw's "ascending ramus," the part that reaches upward to connect with the rest of the skull, as being charateristic of an individual in which "the muscles of mastication ... were of only moderate development;" this condition, he noted, was approached in the skulls of some chimpanzees, but more closely in those of humans.[68] The "horizontal part or body of the jaw" also appeared to show several conditions intermediate between pongids and humans. In regard to those features in which the jaw closely approached those of apes -- such as the so-called "simian shelf" in the anterior part of the jaw and the large root cavity indicating an ape-like, robust canine tooth, Hrdlička contended that such characters could not be "taken as conclusively diagnostic of a chimpanzee nature of the jaw," since it was almost necessary to believe that "the human lower jaw in its evolution must have passed through such stages."[69]

In both this article and in those he wrote specifically on the molar teeth, Hrdlička produced an interpretation of the Piltdown molars that matched his views on the jaw generally. He found the area of the molar crowns, their height, and their pattern of cusps more like "macrodont" humans than like chimpanzees. He also saw strong resemblances between the Piltdown molars and those found among the dryopithecines; the differences between the former and other fossil human molars he considered to be consistent with the "very early age" of Piltdown in relation to other human precursors found up to that point.[70]

The cumulative effect of these observations was to remove the Piltdown jaw from the pongid category and to place it in that of "a human precursor or very early man."[71] The skull was also that of a hominid of course, but Hrdlička thought it quite possible that it had come from a "younger deposit." This "two hominid" theory was Hrdlička's final judgement on the Piltdown issue. He had an opportunity to examine the specimens again in 1925, but his last treatment of the Piltdown fossils, published in 1930, showed that his opinion had not changed much. Both skull and jaw, Hrdlička asserted, were those of hominids, but only the latter was truly primitive in morphology; it was only the thickness of the skull that distinguished it "from a thoroughly modern type of human cranium," and thickness was "an individual, or abnormal, rather than racial character."[72] In his view these facts still rendered the "genetic and chronological association" of the skull and jaw problematic. The circumstances surrounding the excavation of the Piltdown site had, he believed, made exact geological determinations of age difficult, and the fact that both skull and jaw appeared to show a similar degree of mineralization could not substitute for such determinations.[73]

Raising these "chronological" doubts was theoretically quite important for Hrdlička. It was not enough to give the Piltdown skull and jaw to different creatures; one also had to imply that they represented different geological eras, so as to avoid the simultaneous presence of "modern" and "primitive" hominid forms on the same site. Otherwise one would have given more aid and comfort to the early _sapiens_ theory than even the presence of a unified "Eoanthropus" would. Indeed, the manner of handling the supposed pair of Piltdown hominids adopted by Hrdlička came closer to a true instance of "morphological dating" than did his work on human antiquity in the New World.

In 1930, as earlier, Hrdlička saw the dangers implicit in portraying the Piltdown remains as those of a single, geologically ancient form of hominid, but now he expressed them in a direct way that revealed why he had spent so much time on the problem. The theory that Piltdown demonstrated the existence of an early Pleistocene direct ancestor of _Homo sapiens_ was a "superficially attractive" one, he admitted. Still, he felt obliged to counsel his readers against it; "this hypothesis," he warned, "is a proposition that would change the whole face and trend of human prehistory, and that against all other and better substantiated evidence in this line."[74] The fact was, however, that Piltdown, and the interpretations of human phylogeny that placed it in a central role, had already produced the change that Hrdlicka feared. Indeed, he had already laid out his "other and better substantiated evidence" without achieving a deterrent effect.

The "Neanderthal Phase of Man"

The occasion for this exposition had come in 1927, when Hrdlička was invited to deliver the prestigious Huxley Memorial Lecture in London. He truly made the most of that opportunity, producing probably the most lucid, intellectually rich, and powerfully argued essay of his career, "The Neanderthal Phase of Man."[75] Hrdlička clearly knew that he would have to be at his best, for he would have an uphill battle to fight against prevailing opinion, especially in England -- a stronghold of the early _sapiens_ theory and home of both the Piltdown and Galley Hill "fossils." In the years between 1913, when

Hrdlička had first written about fossil hominids in a systematic way, and 1927 opinion had solidified against the Neanderthals. It had become habitual among students of paleoanthropology -- Hrdlička gave illustrations from the writings of Boule, Keith, MacCurdy and others -- to class Neanderthal man as a physically unprogressive and culturally inferior side branch in the human family, and a distinct species which had been totally replaced by Homo sapiens during the last glaciation.[76]

To Hrdlička, the new orthodoxy represented a "position approaching dogmatism" regarding perhaps the "most important period" of prehistory, the Mousterian epoch. This dogmatism, he continued, had had the effect of leading prehistory "into a blind alley, from which so far there has been found no exit, notwithstanding much speculation."[77] The way out of the alley appeared to him to consist in the retracing of mis-steps that from the start had never had adequate light to direct them.

But before he could begin this process of correcting misconceptions, Hrdlička felt that he had to sketch a preliminary definition of "Neanderthal man." On few issues in paleoanthropology over the years have slight differences in definition been more fruitful sources of misunderstanding, and an exact quote from Hrdlička will make the analysis of his argument much clearer: "the only workable definition," he asserted, "of Neanderthal man and period seems to be, for the time being, _the man and period of the Mousterian culture._ An approach to a somatological definition would be feasible, but might for the present be rather prejudiced [emphasis Hrdlička's]."[78] Of course, this identification of a physical population with an archeological "culture" or tool tradition is bad practice according to current canons in paleoanthropology, though it was common enough at the time Hrdlička was speaking. In this case, however, much more than habit was involved in Hrdlička's procedure.[79]

Hrdlička had a very sound reason for not wanting to be boxed into a premature "somatological" portrait of the Neanderthals, and for wanting to retain the widest possible application of the term. The reason was that the standard portrait of the Neanderhal was a too well-marked type, built up, he noted, largely on the basis of the Neanderthal, La Chapelle-aux-Saints and Spy I remains. In defining the type by these, the most extreme specimens in a highly variable group, Hrdlička felt that Boule _et al._ had overdrawn the differences between the Mousterian and later populations of Europe.[80]

Not only in their conception of the physical appearance of the Neanderthals, but also in their characterization of the Mousterian epoch as a whole, Hrdlička believed that the majority had too readily accepted an extreme viewpoint. Where the others saw cultural discontinuity and/or narrow specialization, he saw gradual transition and broad adaptation. From a geological standpoint, Mousterian populations were not, he contended, associated with any one type of climate, and they had been able to persist in several regions over a long span of time. Furthermore, Neanderthal man did not appear to have emerged in, or moved into, Europe with any distinctive fauna, "nor did he move out with any."[81] Also, the Mousterians had not been a population of glacial regions alone -- they had ranged widely through western and central Europe, the Caucasus, North Africa and Asia Minor; nor had they been "cave men" exclusively -- by his count 1/3 of their living sites so far identified had been in the open, while many others had been in shallow caves that had offered no great protection from the elements. Their general level of adaptability thus seemed quite high.[82]

The cultural evidence, Hrdlička argued, showed continuity of development between the Mousterians and their successors that belied the theory of abrupt replacement. Data on human habitation sites that had been complied by MacCurdy[83] showed a trend toward cave dwelling and away from open sites during the entire period reaching from the "Chellean" to the beginning of the Neolithic period; the Mousterian period appeared to have proportions of cave to open sites right in line with the trend. Insofar as food sources and food preparation habits could be reconstructed, the period following the Mousterian, the Aurignacian, seemed to reveal only the introduction of fishing as an improvement on Mousterian practices. The presence of numerous scrapers in Mousterian tool assemblages pointed, according to Hrdlicka, to the preparation of skins for clothing, a cultural trait that had been elaborated further in the Aurignacian period.

Regarding the Middle Paleolithic tool kit generally, instead of abrupt changes at either end of the Mousterian, he contended that "the impression is growing that the more the initial and terminal stages of the Mousterian industry are becoming known, together with the late Acheulian and the earliest Aurignacian, the less abrupt and striking appear the differences and the greater grows the feeling that they are not absolutely separated."[84] This gradual transition in form, he claimed, was confirmed by a wide distribution of sites where the three cultures -- Acheulian, Mousterian and Aurignacian -- were present in adjacent layers and apparently graded into one another.[85]

As the writings of Osborn, and to a lesser degree MacCurdy, indicate, a major prop for the Neanderthal replacement theory was European Upper Paleolithic art, and what it supposedly implied about the intelligence of "Cro-Magnon" man. Hrdlička's critique of this type of argument had three main elements. First, he said it was necessary to remember that Upper Paleolithic art had not appeared in mature form among the early Aurignacians, but rather had taken a long time to develop. Second, he felt that the Mousterians themselves had displayed an incipient aesthetic sense in some of their artifacts, and possibly as well through the use of pigments like manganese oxide, which had recently been found in Mousterian burial sites. Finally, there was no necessary logical connection between the ability to execute realistic cave paintings and great mental power. Many present day primitives, Hrdlička noted, of known artistic and intellectual ability, had never worked in that form; also (turning a typical style of racist argument to an unusual purpose) one of the few groups that did practice a similar style was the "lowly" African Bushman.[86]

Even though the cultural and geological evidence was suggestive, the theories of parallel hominid phyla and abrupt replacement of the Neanderthals also had to be shown incompatible with the <u>fossil</u> evidence. This of course was the part of the problem that was most central to Hrdlička's work in physical anthropology, and his attempt to resolve it drew upon evidence that he had been accumulating for a long time. First of all, the parallel phyla concept was in a weak position because as of 1927 no skeletons had been found in Acheulian cultural contexts, and thus there was no indication that parallel hominid forms had existed prior to the emergence of the Neanderthals. Regarding "Aurignacian" fossils, he noted that though much had been made of the differences between these skeletons and those of Neanderthals, the former were few in number and "essentially" from middle and late rather than "the most needed early Aurignacian."[87] Not only did a gap exist that could easily have been occupied by a transitional form, but also, in reviewing all of the Mousterian fossils extant, the possibility of a gradual transition was enhanced by the discovery that much more variability existed in that group than the proponents of an extreme Neanderthal "type" had implied.

The last point contained the critical factual contention in Hrdlička's argument, and he developed it in detail. He reminded his audience of the well-known ensemble of characters that constituted the "Neanderthal type" -- i.e. "the flatness of his head, with low receding forehead and a peculiar protruding occiput; heavy, supraorbital torus; heavy, chinless jaw; and as determined from intracranial casts, a low type of brain."[88] Yet, said Hrdlička, when all the extant Mousterian skulls were examined, there was found "a large range of gradation, the lower limits of which are well below, but the upper grades of which are well within, the range of variation of the same characters in later, and even present, man."[89] Just as he had back in 1913, Hrdlička cited the Spy I and Spy II skulls as examples. Though they had been found at the same level and "but six feet apart" only the first conformed to the "type"; the second, he claimed, was "so superior in size, shape, and height of the vault, and height of the forehead" that the "morphological distance" between them seemed greater than that between Spy II and certain "Aurignacian crania."

A similar approximation to conditions found in Homo sapiens was in his view displayed in the skull vaults of the two most recently described Neanderthaloid finds -- the Galilee and Ehringsdorf skulls, an apparent modernity that even extended to the brain casts of the fossils. Indeed, for the Galilee brain's progressive characters Hrdlička could, surprisingly, quote the authority of Sir Arthur Keith.[90] From another direction, Hrdlička could support his thesis on this issue by citing individual Asian and American Indian crania from his collections in the Smithsonian, crania which approached the less extreme Neanderthal skulls in lowness of the skull vault and the protrusion of the occipital region.[91]

The same kind of evidence, Hrdlička believed, emerged from an analysis of the jaw and supraorbital regions as well. The most extreme examples of the Neanderthal "type" had characters that were very difficult to match in later human populations, but there were Upper Paleolithic skulls that were transitional between Neanderthal and modern forms in these characters, and certain Upper Paleolithic and even modern skulls approached conditions in the "atypical" Neanderthalers. The same story, he said, could also be repeated with post cranial remains -- the "type" was well represented in the Neanderthal and La Chapelle-aux-Saints skeletons, but much of the skeletal material found at Krapina, LaFerassie and La Quina could "approach to, or merge with the modern."[92]

To Hrdlička, then, the physical evidence rendered the hypothesis of abrupt replacement and extinction of the Neanderthals dubious. He also questioned the plausibility of that scenario on theoretical grounds. The normal picture presented in this scenario was that of an invasion of Neanderthal-occupied regions by the culturally superior

Aurignacians, who either killed their hapless predecessors, or outcompeted them for available resources. Where, Hrdlička asked, had these invaders come from? If from Africa or the Near East, the usual suggestions, why had they moved northward into the cold environs of Europe during a severe glacial epoch? In his view such a migration would have been contrary to the tendency of all later population movements.[93]

Perhaps more important, Hrdlička questioned whether the large numbers of "Aurignacians" necessary to displace the Neanderthals without the occurence of intermingling and hybridization could have been able to move in and command the available resources without complex cultural traits such as the bow and arrow, or domesticated animals. The issue he was raising was a critical one that had been sloughed over too often -- i.e. could a people on a simple Paleolithic plane of culture really exterminate or rapidly crowd out another people only slightly less advanced? Though in his own mind this point was probably subsidiary to his morphological arguments, it was actually a crucial one, for it questioned the relevance of modern racial analogies, such as the colonial era's conflict betwen Englishmen and Australian aborigines, that were relied on implicitly by the replacement theorists.[94]

Even if Aurignacian man had been capable of something approaching genocide, the problem still remained of where this form of human had evolved while the Neanderthals had been in possession of Europe and its environs. Why, wondered Hrdlička, had no solid evidence of Homo sapiens of the same age as Neanderthal man been uncovered either in Europe or elsewhere? Under what conditions different from those faced by the Mousterians had this superior form evolved? Also, what creature was the ancestor of this early Homo sapiens ? Outright "polygeny" -- Hrdlička's old-fashioned term for the notion that Homo sapiens and "Homo neanderthalensis" had evolved from quite different types of early hominid, seemed both "undemonstrable and improbable." If, however, both had evolved from the same ancestor, there seemed to be no explanation for the differences in the rates of evolution experienced by the respective lineages. Why should one line have evolved quickly into Homo sapiens, and then have undergone very little change since well back into the Pleistocene, while the other line showed relatively little directional change in its long career, and became suddenly extinct. For a generation of biologists whose view of evolutionary change tended to see gradual and "orthogenetic" change as the normal course in mammalian evolution, these should have been powerful arguments against the replacement idea.[95]

Having roundly attacked the idea that Neanderthal man was a dead side-branch of the human family tree, Hrdlička was ready to lay out the evolutionary scenario that embodied his alternative -- the theory that there had been a Neanderthal "phase" in human emergence. What did he mean by this term? At the end of his address he spoke bluntly of "the evolution of the Neanderthalers into later man." The connotations of this phrase made it a convenient point of attack, for it could be read as if Hrdlička was designating the "classic" Neanderthals of western Europe as the sole source of modern Homo sapiens.

The thesis being presented was considerably more subtle than this, however. Hrdlička in fact argued that if one took all of the Neanderthals, i.e. the fossils associated with the Mousterian culture, as a group, one found that their main differences from both Upper Paleolithic and recent populations could be bridged by changes falling into two broad categories: "(1) reduction in musculature -- that of the jaws as well as that of the body -- with consequent changes in the teeth, jaws, face, and vault of the skull; and (2) changes in the supraorbital torus of the order well known to morphology as progressive infantilism."[96] Both of these categories of change were highly significant because they could be seen to be parts of trends that had also continued in <u>Homo sapiens</u> from Upper Paleolithic times into the historic period; in addition, differences existed among the existing Neanderthal fossils themselves in the degree to which these changes had progressed.

The conception of continuous morphological trends fit in with Hrdlička's long held belief that similar forces of function and adaptation had shaped both recent humanity and its precursors. The diversity within the Mousterian population seemed to Hrdlička "a very noteworthy example of morphological instability, evidently of evolutionary nature, leading from old forms to more modern," a similar interpretation to that which Keith and McCown were soon to give regarding the variability in the Mt. Carmel population of Palestine. In Hrdlička's view, the explanation of this instability was that "a relatively rapid, progessive change, both mental and physical, was actually taking place" during the Mousterian period.[97] This last option was also one that Hrdlička had long believed in, and it reveals especially well how the ideas in the "Neanderthal Phase" were in harmony with, as well as firmly built upon, his earlier work.[98]

In the attempt to account for these "progressive changes" Hrdlička, in common with his contemporaries, started from the assumption that the populations which had followed the Mousterians were probably more efficient physically, and assuredly so mentally, than the latter. The scenario he presented started with the idea that the slow intensification of the last major glaciation had put increasing pressure on human populations. The "great changes of environment" that man encountered supposedly had included harder winters as well as important faunal changes; human culture had had to become more sophisticated in order to provide "more shelter, more food, more fire, and storage of provisions," as well as "new adaptations and developments in hunting." In addition, he conjectured, environmental change had probably increased the impact of respiratory and other diseases, thus "hindering the growth of the population."[99]

These environmentally induced challenges, Hrdlicka believed, had had two major results -- "an intensification of natural selection," and "greater mental and physical exertion" by human beings. In Hrdlička's still partially Lamarckian view of evolution these two processes, acting in tandem, provided the "very essentials of progressive evolution." Increased exertion, "where not over the normal limits" brought "greater efficiency attended by further bodily and mental development," while natural selection would eliminate those who could not adapt quickly enough.[100]

Not only would some individuals have fallen by the wayside in this process of progressive evolution, Hrdlicka contended, but also in some regions conditions would have been more favorable for advance, while in others "disease, famine, and warfare" would have overcome an overstressed population and extinguished it. As adverse conditions intensified toward the height of the last glaciation the aggregate human population would have become ever smaller, with only the "most fit or able-to-cope-with-the-conditions group or groups" able to survive. This picture, Hrdlička concluded, contained a "relatively simple, natural explanation" of the progressive evolution of Neanderthal man, a process that would "inevitably" have carried the latter's "most advanced forms to those of primitive Homo sapiens.[101]

As this detailed recapitulation of the argument reveals, Hrdlička was suggesting a more complex picture than the words "Neanderthal phase" of man might have led one to expect. In order to underline this fact he drew four phylogenetic trees, each illustrating a different conception of the relationship between Neanderthal and present man (see Figure 1). The most important contrast for present purposes is the one between the tree on the extreme left and that on the extreme right, which Hrdlička deemed the correct one. By distinguishing these two from

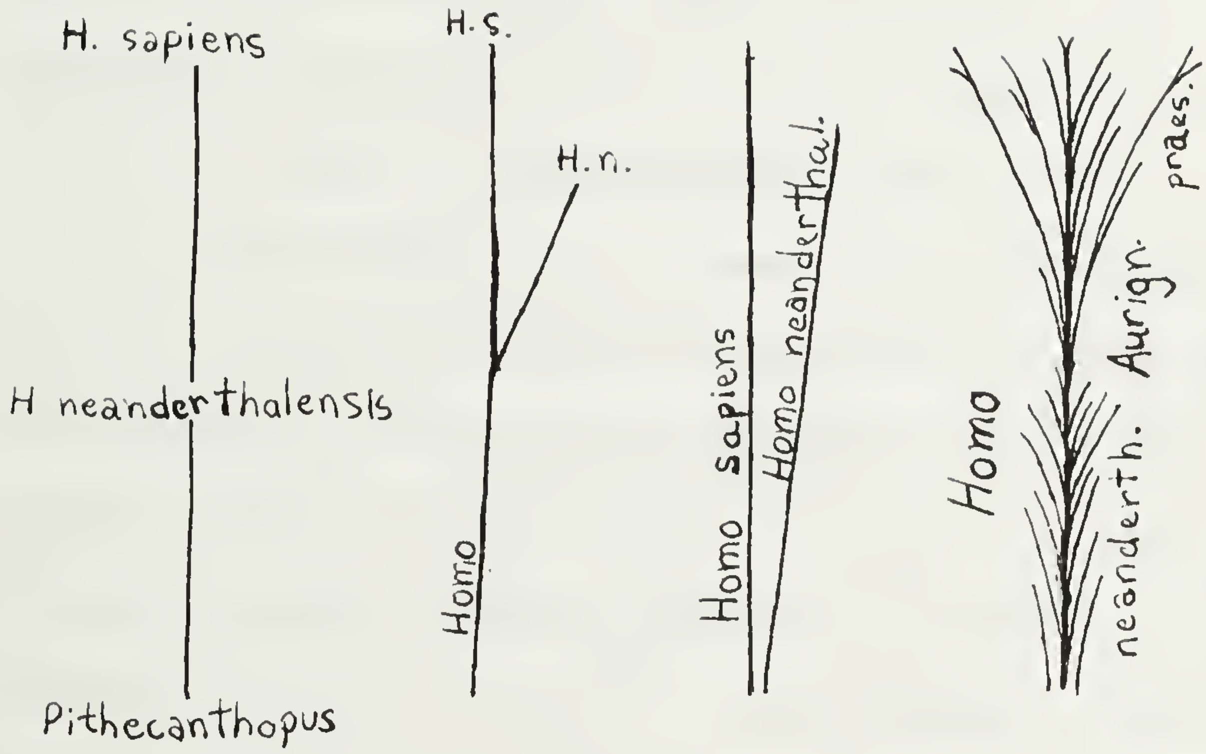

Figure 1. Differing versions of the phylogenetic relationship between the Neanderthals and anatomically modern Homo Sapiens. After Hrdlicka, "The Neanderthal Phase of Man."

each other he meant to exclude the implication that _all_ forms of Neanderthal man had been ancestral to the modern human species. Instead, he was saying that only the more advanced, or well adapted, representatives of the former group would have been on the main stem at any point in time. The others, or their immediate progeny would have come to the various dead ends indicated. The rather inelegant illustration was meant to show the complexities of this process of "progressive" evolution.

In the "Neanderthal Phase of Man" Hrdlička had clearly fashioned a powerful critique of what had become the orthodox view about the Neanderthals. Was his own "simple, natural explanation" of the evidence, however, a solid and cogent alternative in the context of the times in which it was written?[102] There appear to be several aspects of the hypothesis that might have deterred Hrdlička's contemporaries and immediate successors from accepting it. Most obviously, while it succinctly reduced the _major_ differences between the Neanderthal and later populations to those of greater robustness of jaws, skull and musculature in the former, and increased "infantilism" in the latter, it did not specifically remove the "stigmas" of excessive primitiveness and probable specialization created by Boule's work on the Neanderthals. Not taking on Boule's characterization of the "type" more directly was a tactical mistake, if one's purpose were to change people's minds about the western European Neanderthals.[103]

Also, in regard to the trend toward muscular reduction and "infantilism" that he identified, Hrdlička did not spell out the way in which that trend was adaptive under the environmental conditions he portrayed. Related theoretical difficulties were the implied Lamarckianism of his explanation of this morphological trend, and his idea that "instability" was a sign that progressive change was occuring. While he was not alone at the time in holding either view,[104] both were questionable even then, and were bound to make his views appear antiquated as paleoanthropologists began to absorb the lessons of Mendelian genetics in the not too distant future.

Another problem related to trends within the long Mousterian period. In Hrdlička's address the focus was on the broad issue of variability within the fossil population found in Mousterian contexts, a focus which was tied in with the need to demonstrate "instability;" for that reason he did not try to delineate patterns of change over time or regional differences among the Neanderthals. To put the case of "progressive evolution" convincingly to a sceptical audience, though, it would probably have been necessary for him to identify at least the former pattern.

Hrdlicka did ease the burden of proof somewhat by noting that many of the Mousterian-related fossils were not securely dated relative to one another, making exact determinations of such matters difficult.[105] This vagueness in dating in fact made it possible for him to imply that the morphologically most primitive specimens might have been the oldest.[106] Unfortunately, this implication would not hold up under the weight of continuing discoveries and the way they were to be interpreted. Thus, by the mid-1930s it was accepted that the Ehringsdorf and Steinheim skulls, in several ways the most "progressive"-looking of the European Neanderthaloids found up to that point, were actually earlier than the principal fossils of "classic" appearance. Indeed, it was at first believed that the Mt. Carmel skeletons were earlier as well.[107]

Hrdlička's picture of environmental change as a cause of progressive evolution was an interesting one, but it created difficulties for him as well. It was Europe-centered, like his earlier theorizing on human dispersal, and finds of "progressive" Neanderthaloid or anatomically modern-looking fossils from areas where glaciation could not be considered a major evolutionary factor were bound to render it suspect. Such finds were not long in coming -- e.g. the Skhūl cave specimens from

Mount Carmel found in the early 1930s. Finally, Hrdlička's unorthodox views on European glacial chronology would have diminished the credibility of any evolutionary scenario that relied heavily upon them.[108]

All of the foregoing might help to show why Hrdlička's address, though respectfully received, made little impact on the paleoanthropological community. There was more involved, though, than the way in which he presented his argument. Another difficulty stemmed from the fact that Hrdlička was not reasoning from a new body of evidence. Most of the data on which he based his hypothesis was already in the "public domain," and ideas about that data had pretty much solidified. This was a major drawback in a field in which the initial describers of new material are given special attention for their theoretical speculations, the assumption evidently being that new fossils provide the best platform from which to launch new evolutionary scenarios. In the period between 1905 and 1915, Boule and to a lesser extent Keith had laid the foundation for the Neanderthal replacement theory in studies upon what was then a relatively new body of evidence about the morphology of later Pleistocene fossil hominids. In 1927, Hrdlička was trying to use roughly the same base to erect a new edifice, but it was an edifice that must have struck many in his audience as belonging to an outmoded style.

Hrdlička's omission in his lecture of the troublesome Piltdown issue and its relation to the theory of parallel phyla probably did not help him either. That his viewpoint on "Eoanthropus" repesented a minority position was probably well known, but not showing how that view was central to the matter at hand was probably a serious tactical error. Piltdown man, if not discounted, was bound to figure in the response to Hrdlička's message as a persuasive point against what he was saying and in favor of the early appearance of Homo sapiens. [109]

Questions of impact aside, "The Neanderthal Phase of Man" was the culmination of Hrdlička's work in paleoanthropolgy, and his greatest contribution to the discipline. He was correct in arguing that the study of human evolution had tended to move into a "blind alley" in his day. The emphasis on racial analogies, the strong appeal of polyphyletic scenarios, and the neglect of the role of biocultural adaptation in hominid phylogeny had all worked together to produce this relative sterility in the discipline of paleoanthropology. Many of the criticisms that he made of competing theories were to the point, and recent analysis of the problem indicates that several of Hrdlička's ideas about the later stages of human emergence remain fruitful. Far from being daunted by his failure to change people's minds, Hrdlička maintained his commitment to the theory of a Neanderthal stage of evolution with its center in Europe. In fact defending that theory was a central preoccupation of his last major work on fossil hominids -- The Skeletal Remains of Early Man, which appeared in 1931.

"Holding One's Own" -- Hrdlicka's Later Views

The Skeletal Remains of Early Man lent a certain symmetry to Hrdlička's involvement in the study of paleoanthropology; like his first major contribution to the subject, it was a comprehensive description and analysis of all the major discoveries made to date, which in this case was December, 1929. As indicated above, Hrdlička devoted considerable attention in this work to the "Neanderthal phase" idea and the principal arguments in support of it, but its effect was to provide documentation of the points he had made in 1927, not to add anything substantive to them.[111] More interesting was the way in which he tried to harmonize the data on other specimens with the conclusions derived from his work on the Mousterian and Upper Paleolithic populations. His defense of the "two hominid" interpretation of Piltdown has already been noted.[112] Another troublesome specimen that he handled in a way that was reminiscent of his previous discussions was "Pithecanthropus."

As earlier, the keynote of Hrdlička's attitude toward Java man was ambivalence.[113] On the one hand, he argued, the skull cap of the creature, while resembling anthropoid apes in some ways, had progressed far in the "humanoid direction." This impression was more than confirmed, he believed, by analysis of the interior of the skull vault, which, he pointed out, had only recently been freed from the rock which adhered to it when it was unearthed. The resulting endocranial cast revealed a brain whose "size and form and gyration" appeared to "remove it at once from the brains of all known apes and bring it correspondingly closer to that of man." In fact, said Hrdlička, this brain was "inconsistent and morphologically superior to its own skull." Given the likelihood that the skull cap was that of a female, a male "Pithecanthropus" would probably have had an endocranial cast of about 1100 cc., dimensions which would, he noted, "connect already with the human."[114] Indeed, he continued, if this form had "advanced in brain size and form by again as much as it had already stood above" the brains of known apes, it would have been "wholly impossible to exclude it from the human category, unless it was done by the establishment of a separate genus of creatures, equivalent in brain mass and differentiation to Homo."[115]

Since the latter procedure was exactly the sort that Hrdlička tended to criticize when applied to later forms of humanity, one might expect Hrdlička would be eager to grant Java man a position in human phylogeny, since what was known of its skull form, and believed about its brain, was fully in line with the morphological trend leading from Neanderthals to later humans. But probably because of the confusion that that would bring into the issue of geographical centers of human evolution, Hrdlička held back. His final conclusion on "Pithecanthropus" was to suspend phylogenetic judgement for the time being, pending "further and conclusive evidence." His provisional conclusion would only be to regard it as "a high Primate of as yet uncertain ancestry and no known progeny, far advanced in what may be termed a humanoid direction."[116]

A fossil find of the 1920s to which Hrdlička gave considerable attention in Skeletal Remains, and one that posed serious difficulties of interpretation for him, was the so-called "Rhodesian" or "Broken Hill" skeleton. Some writers in the period just after its discovery had classified "Rhodesian man" as an African

Neanderthaloid,[117] on the basis of the general similarity that it had to the latter in characters like a large supraorbital torus, "prognathic" facial region and low forehead. For Hrdlička this simple solution was unacceptable. First, the presence of a representative of the "Neanderthal phase" so far south in the African continent would present complications in the geographical pattern of human evolution similar to those implied by a Java man evolving toward advanced hominid form. Furthermore, the details of the "Broken Hill" man's morphology created a discordant impression in his mind, for it seemed to possess an odd combination of advanced and primitive characters that violated his conception of gradual, progressive change.

Much of the "discordance" Hrdlička saw was in the reasonably complete skull of the Rhodesian specimen. "The frontal and most of the facial parts," he said, "exceed in primitiveness every other known specimen of primitive man." By contrast, the skullcap "from behind the frontal ridges" he saw as "of a decidedly higher grade equalling in many respects and in some even exceeding those of the more typical Neanderthal crania."[118] Going through a number of individual characters he found some to be "pre-Neanderthaloid," others to be "Neanderthaloid," and still others to be "recent" in appearance. Indeed, he even suggested that some in the last category of characters -- "the diminishing third molars, the shape and size of the other teeth, the extensive caries, and other points" -- spoke to him "strongly against hoary antiquity" for the skull,[119] though he was sure (because there were too many "primitive" characters) that this was not a case of atavism or "reversion."

While Hrdlička did not know how old the skull was, because of geological as well as these morphological uncertainties, he did seem to accept it as an authentic Pleistocene fossil. Not so with the tibia and other human bones found with the skull. Apparently, these did not suggest the picture of short stature and highly developed musculature that he had come to expect of early hominids as a result of the Neanderthal populations he knew so well. Therefore, contending that there was "no proof, and but a remote possibility, of any of them belonging to the skull," he sugested that these bones had come from "several skeletons of modern size and form."[120]
Having disposed of the postcranial material by the sort of geological double standard that he applied in the Piltdown case as well, Hrdlička give the following somewhat whimsical judgement on the skull as a whole: it was a "tantalizing specimen" but he was "wholly at a loss as to where" it belonged "taxonomically or chronologically."

"It is" he mused, "a comet of prehistory."[121]

Hrdlička's difficulties with the Rhodesian skeleton were understandable. The only fossil remains known at the time that were clearly more primitive, morphologically and geologically, than the Neanderthals were the "Pithecanthropus" skull cap and the Heidelberg lower jaw, and both were fragments, so the basis for meaningful comparisons was narrow. To accept Rhodesian man as of similar or greater antiquity compared to the Neanderthals, and/or to accept the postcranial bones as associated with the skull would open up questions like those of multiple centers of hominid evolution and possible phyletic specializations in these different hominid groups. With hindsight, it is easy to see that what would have helped Hrdlička most to "get his bearings" on these issues was a complete skeleton of <u>Homo erectus.</u> The irony of the situation was that by 1929 a reasonably complete skull from this species had been found, and that Hrdlicka rushed to a hasty judgement of the skull that contrasted greatly with his hesitancy over the Broken Hill remains.

The skull, of course, was that of "Sinanthropus pekinensis." Though its discoverer, Davidson Black, had not yet published a full analysis of the first adult specimen of the form, he had sent a preliminary description and photographs to Hrdlička. The latter used these to put together an "addendum" to Skeletal Remains, and the conclusions Hrdlička reached were not of a sort with which Black could have been pleased. For one thing, Hrdlička pointed out with some irony that the Peking finds had finally revealed "the presence of man much farther east" than he had been "previously known or legitimately suspected." This clearly was a swipe at the "mere ideas" of Osborn, and by extension Black himself, who had predicted the existence of early hominids in Asia prior to his discoveries.[122] Even more distressing than this, however, was the provisional phylogenetic judgement Hrdlička gave. Where Black saw a new, primitive genus of hominids, Hrdlička saw the following:[123]

> The lower jaw resembles very closely No. "g" of Krapina, and the plainly diminished third molar suggests a moderate rather than extreme antiquity. The skull is clearly neanderthaloid. It appears to represent no distinct genus, species or even a pronounced variety. And it is not like the lowest type of the Neanderthaler, but corresponds rather to the better developed specimens of that class, such as the Galilee skull. How far these still necessarily provisional views may be substantiated, remains to be seen.

The hint of a concession contained in the last sentence was short-lived. In a brief "note" following his "addendum" Hrdlička said that he had received additional photographs from Black, and that these photographs had "greatly" strengthened his initial judgements. In fact, his last word on the issue was to claim boldly that "had the skull been found in Europe or in Asia Minor, it could hardly be taken by any expert as anything else than Neanderthaler."[124]

This episode apparently led to a permanent cooling in what had been a cordial relationship between Hrdlicka and Black.[125] The personal reasons behind the haste and apparent vehemence of Hrdlička's judgement can only be guessed at. Intellectually, though, the threat to Hrdlička's long held ideas represented by "Sinanthropus" can be seen as a serious one. First, if "Sinanthropus" were indeed a more primitive form than the Neanderthal, it would put two, and as Black believed very similar, early hominids on the Asian continent -- surely a great blow to the theory that Europe was the most likely center for human evolution and dispersal. Of course, the term "dispersal" implied a second danger -- an Asian center of dispersal would have struck the strongest blow yet to his theory accounting for the late peopling of the Americas. The latter issue was, after all, the one around which he had built his reputation as a student of early human populations, and to which he was still strongly committed. Finally, as we have seen earlier, so much of his argument on both these issues was built on the absence of positive evidence for the alternatives. The discovery at Choukoutien, even more than that at Broken Hill, probably seemed to presage an uncertain future for his entire conception of the human past.

The Skeletal Remains of Early Man was Hrdlička's last significant work on the phylogeny of fossil hominids and the geological origins of modern humans. For this reason it makes sense to close our treatment of these themes in Hrdlička's writings with it. That he continued to subscribe to the most important idea in later work -- the "Neanderthal phase" hypothesis -- is suggested by the last notice that he ever published on a fossil find, a 1939 report in Science on the "Teshik-Tash" skeleton. This fossil, which had been discovered by Soviet scientists working in the central Asian Soviet republic of Uzbekistan, was that of a juvenile; it had been found in a Mousterian cultural context, and it was very similar to European Neanderthal skeletons of similar age. In his brief reflections on the meaning of the fossil Hrdlička could not resist the ironical comment that "thus, unexpectedly, central Asia furnishes a first rate piece of evidence of early man." Nevertheless, he saw the importance of that evidence, saying that it would necessitate "a material revision of notions relating to the Neanderthal phase of human antiquity." Previously, he noted, the easternmost finds of Mousterian artifacts or Neanderthal fossils had been in Crimea, Palestine, and the

Caucasus; now he asserted, the Teshik-Tash find "halves the distance from the western Neanderthalers to Peking Man."[126]

These somewhat cryptic utterances do not indicate clearly the direction that Hrdlička would have taken in such a "revision." Probably a Eurasian-rather than merely European-based "Neanderthal phase" was what he was implying. The phrase "halves the distance" appeared to indicate that he was still subsuming Peking man under the Neanderthal rubric. At any rate, he in no way indicated that the "material" revision would ever have included abandonment of the concept. More likely, it was the ideas of his opponents that he felt to be in need of major overhaul, now that "Neanderthaloid" peoples had been shown to have existed throughout much of the Old World. At any rate, nothing in the later writings of Hrdlička indicates that he differed significantly from either MacCurdy or Hooton in the way he used the discoveries of the late 1920s and 1930s -- i.e. by interpreting them so that they caused the least disturbance to views worked out earlier.

General Themes in Hrdlička's Portrayal of the Evolutionary Process

The discussion of Hrdlička's ideas on human evolution would not be complete if we did not devote some attention to his more general writings on evolutionary processes and race, as well as to those that reveal his attitude toward some of the basic methodological issues in physical anthropology. Examining these will not only clarify our understanding of the unique presepctive that he brought to the study of human emergence, but will also give some insight into intellectual problems that the emerging science of physical anthropology had to face in the early years of this century. The focus here will not be, as it was earlier, on how ideas developed over time, but rather on outlining major themes that seem to be relatively consistent in Hrdlička's mature writings.

In general, Hrdlička's writings on these subjects reveal what is to the modern reader an unusual juxtaposition of a critical, and often subtle, appreciation of the strengths and weaknesses of certain traditional ideas with a stubborn adherence to others. In some ways Hrdlička's general outlook can be compared with Hooton's, but perhaps in line with the age difference between them (nearly twenty years) several of the

Hrdlicka's "hobby horses" were more characteristic of the late 19th century, the period in which the latter reached intellectual maturity.

Examples of the former, critical attitude are common in Hrdlička's articles on evolutionary processes. Much like Hooton and Gregory, he was suspicious of some of the "laws" of evolution that biologists of Osborn's stripe often invoked. He cautioned, for example, against the rigidity of certain versions of "orthogenesis," contending that though evolution had been progressive -- in the sense of leading "unceasingly to progress in diversity, complexity, sensibility, and effectiveness," it should not be seen as a "pre-ordained" unfolding of some sort of inbuilt potentiality.[127] A similar flexibility, he thought, ought to exist in the way scientists understood the "law of irreversibility" of evolution. He believed the principle to be valid if one meant to rule out the reversion of a creature to an exact repetition of an ancestral form, but one must also keep in mind that the guiding principle of adaptation to environment could make an approach toward an ancestral type possible, especially if it involved simplification of structure.[128]

Hrdlička displayed a similar caution in certain of his views about heredity and genetics. He could, for example, credit the idea (which was a popular, though by no means universally accepted one at the time) that evolution could take place by means of deVriesian "macro-mutations" only with qualifications; while he thought this process did occur from time to time, he saw it as clearly subordinate, especially among higher animals, to evolution by more easily assimilated small changes in structure and function.[129] Though Hrdlička accepted Mendelian gene theory, he warned against the excesses of what later critics have termed "bean bag genetics" -- the notion (common especially among early 20th century eugenicists) that most important human characters could be traced to single genes that acted in all or none, dominant-recessive fashion.[130] His awareness of the genetic thinking of his day came through in references to the mechanisms of addition, inhibition, and suppression by which genes affected each other, as well as to the subtle effects that the maturational process could have on the expression of genetic potentialities.[131] It must be noted, though, that this awareness of modern Mendelian ideas did not enter directly into his work on the previous stages of human evolution.

Hrdlička's emphasis on the importance of the study of variation in anthropology provides an even better instance of the careful, critical nature of much of his work. He firmly believed that what distinguished physical anthropology from the other biological sciences was the importance of its comparative element. Anthropology, he asserted, "is comparative human anatomy, physiology, psychology, sociology, linguistics, etc." And being comparative, it did not deal with "individuals or with mere abstract averages, but with groups of mankind." In brief, it was "the science of human variation, both in man himself and his activities."[132] He was not dealing in vague exhortations when he set the study of variation as the agenda for the discipline; as his biographer points out, and as the story of his study of the human dentition illustrates, Hrdlička spent a great deal of his time as a scientist trying to fix the limits of "normal variation" in the populations he studied.[133]

In his pursuit of the study of variation, Hrdlička was also able partly to shed (perhaps to a greater degree than Hooton had[134]) the contempt for non-literate and non-white human groups that was a common theme in late 19th and early 20th century biology. While he believed that the first goal of physical anthropology in his day was to provide as complete an analysis as possible of the biological condition of the white race, he also saw as necessary the undertaking of similar studies on "the more primitive groups" of humanity. This should be done, he argued,[134]

> not alone for descriptive and statistical purposes, but for a proper understanding of our own race and of humanity in general. The more primitive groups of people are less mixed, less abnormal, less pathological, perhaps less aberrant than those of more civilized communities, hence observations thereon may reasonably be expected to reveal more readily and clearly the workings of natural laws that control man's cycle of life, his adaptations, his changes, and his evolution.

Of course, Hrdlička did his best to enlarge the stock of biological data on these "more primitive groups," especially those residing in America and northern Asia. As the passage above indicates, however, Hrdlička was not just interested in collecting data for its own sake, but also to discover evidence of "natural laws" at work in the human populations he studied. Perhaps his greatest claim on the status of a "modern" figure in physical anthropology is the way in which he tried to go beyond the attempt to define racial and other physical "types" and get at the evolutionary processes he believed to be at work among recent as well as fossil humans. And in his view the same processes could be seen creating trends that ran from the prehistoric past right through to the present, as his studies on the teeth, and especially the "shovel-shaped" incisor illustrate.[135]

Alongside these "modern" or "forward-looking" characteristics, Hrdlička also managed to display a number of traditional and even antiquated ideas in his work as well. While he promoted the importance of the study of variation, for example, he was not comfortable with the newer means of analyzing it -- statistics. He eschewed the standard deviation, probable error and all the other paraphernalia of the biometrical school, and stuck to arithmetical means and simple proportional indices (such as the tried and true "cephalic index").[136] Though, as can be seen in the case of Hooton, new methods could easily be adapted to conservative purposes, Hrdlička's reluctance to adopt them probably placed him at a disadvantage in retaining a long lasting professional audience.

Sometimes Hrdlička dispensed with mathematical analysis entirely and relied on his morphologist's eye for discerning form. In his use of qualitatively defined morphological "types" he thus helped keep the traditional "craniology" pioneered by the Broca School alive. For an example one could cite an analysis he did of a type he called the "full-blood American Negro." While he was able to note several quantifiable ways in which this personage came up short in comparison with a sample of white "Old Americans," he nowhere revealed his criteria for assigning Negroes to the "full-blood" category -- an omission which tends to indicate that the Negro sample was selected on the basis of appearance, which would have made the "scientifically" measured type the product of an idealized preconception. That he believed firmly in the existence of discrete types is also indicated by his attempt to provide a general theory of how these types came into being and how they were perpetuated, with an application of the theory to the character of head form.[137]

In addition, again conforming to tradition in physical anthropology, Hrdlička revealed a belief in racial mental types. In an essay on "Human Races," while stressing, as Hooton also did, that the mental differences between races had thus far eluded "direct and precise specification or determination," he nevertheless went on to produce a catalogue of racial mental characteristics -- e.g. Mongoloids were "mostly less vivacious and temperamental" than whites, while Negroes were "not very ambitious" and "less rational" than whites, though their "emotions and passions [were] strong." [138] Hrdlička, like Hooton, also indulged at times in the practice of reconstructing the history of racial migrations through anthropometric analysis of supposedly racially "mixed" present populations.[139]

As one might expect of a writer who made use of traditional racial typologies, not to say stereotypes, Hrdlicka was also willing to entertain the idea that living races displayed differences in levels of intelligence of an evolutionary nature. Like Hooton, he prefaced discussion of this dangerous topic with a proper note of caution; the "general view" of racial inequalities in mental ability was, he warned, less the result of "thorough scientific investigation" than of "more or less subconscious feelings due to accumulated bias and experiences" as well as to "egoism and ignorance."[140] As a Czech immigrant, and one proud of his heritage, he must often have rankled at the nativist and "Nordic supremacist" attitudes that were so common in his day, even among professional biologists.[141]

Yet despite his sensitivity to these excesses, he accepted the notion that there were mentally "advanced" and "belated" races -- even endorsing the time honored idea that temperate climates had stimulated mental evolution, while tropical regions, which presented little in the way of mental challenge, had kept the races inhabiting them on a lower mental plane.[142] Specifically, Hrdlička contended that studies of the brain, though "far from finished" had shown that Whites and Negroes, the main races representing the temperate-tropical distinction, did indeed lie on different planes of mental capacity.[143] Also, he thought that such differences would remain, even as the human race progressed in intelligence in coming generations: "so far as can be discerned," he said, "there is no promise of eventual equality of the races ... There will always be masters and servants, pioneers of progress and the dregs."[144]

Overt racism was of course common in scientists of Hrdlička's generation, and compared to scientific proponents of eugenics like Osborn, it was a much less dominant theme in Hrdlička's thought. Some of his other research interests, however, seemed to have more of a late 19th century air about them than was the rule in the era between the two world wars. Perhaps the most interesting, his research on "Children Running on All Fours," represented an attempt to identify an example of recapitulation in humans. After documenting thirty-three cases of temporary quadrupedal progression in children, he found that three-fourths of the time it substituted for the "creeping" (on hands and knees) mode displayed by most children prior to walking. As there seemed to be no other abnormality in such children, the "basic cause", he concluded, was "apparently of atavistic nature, the whole phenomenon being thus one of the order of functional reminiscences of ancestral condition."[145] He seemed to be interested in "atavisms" like this one and others such as hirsuteness and supernumerary nipples primarily because he took them to be evidence of human descent from lower mammals, though he did not use them as a base for specific phylogenetic conclusions.[146]

Two other studies that seemed to reflect the perspective of an earlier generation appeared in the years just after Hrdlička's work on quadrupedal children. In one, he undertook to test the received physiognomic wisdom that men with high foreheads possessed greater than average brain power. After measuring living individuals from various ethnic, regional, and occupational groups, including members of the National Academy of Sciences (to which he belonged), he was able to conclude with confidence that "the lowness or height of the forehead, in normal human beings, does not express or have any relation to the kind of brain it helps to harbor."[147] A companion study, which analyzed several kinds of anthropometric data collected from Academy members, found these intellectually successful individuals as a whole to be "superiors in physique, strength, health, and longevity, as compared to the American or even the old American population at large." Specifically, in head size relative to stature, which he took to be an indicator of superior brain size, these scientists surpassed even "the well-educated and professional old Americans," which Hrdlička's own extensive studies had shown to be an "excellent stock."[148] Whether such studies turned up anything of scientific importance is not at issue here; what does seem clear is that the late 19th century French anthropologists from whom Hrdlička learned his craft had considered such studies of the physiognomy, craniometry and constitution of "men of eminence" to be of great interest. Hrdlička was thus keeping a traditional, not to say "old-fashioned," subject alive when he conducted them on American subjects.[149]

As interesting as these studies are for showing the kinds of problem that Hrdlička concerned himself with, his commitment to Neo-Lamarckianism was probably the thing that did the most to "date" his ideas. We have already seen this viewpoint represented in his work on fossil hominids, but it also recurs as a major theme in his theoretical essays on human evolution, even as late as the year 1942.[150] In one essay, the way he phrased his belief was reminiscent of James Mark Baldwin's principle of "organic selection": "It may be stated, as an organic law," he wrote:[151]

> that every reaction, whether in the direction of more or less, unless artificially counteracted, leads, if repeated often enough and within healthy limits, to an organic habit and organic modification. And such habits in the course of time lead, in some way that is as yet not fully understood, to more or less hereditary traits -- which are items of evolution or devolution.

At other times Hrdlička opted for a more or less traditional version of use inheritance, though he did try to accomodate the objections to that principle that had been raised by the studies of August Weismann (1834-1914), when he used words like the following:[152]

> acquired characteristics, the influence of which does not reach deep enough to the trophic centers of the brain or nervous system and the germ cells, are as a rule not inherited. But there are many functional acquirements that evidently in time do reach these depths, as a result of which they tend to become fixed and hereditary.

What exactly Hrdlicka meant by "trophic centers" he did not explain in detail, nor was the mechanism delineated by which the genes and chromosomes of the "germ cell" were modified. His study of heredity had made him aware that older ideas like Darwin's "corpuscular" theory of use inheritance were unsupported. He still believed, however, that some sort of "chemical effects," or perhaps "nervous and other radiant impulses" were able to produce the genetic changes he felt had to occur.[153] Because he had a limited concept of mutation (he defined the latter in the DeVriesian sense of major, discontinuous shifts in hereditary endowment) he almost had to put faith in the inheritance of small functional changes first acquired through exercise. Otherwise phenomena like "orthogenesis," or "progression in a part or organ in a given direction," especially orthogenesis "in the directions of greater complexity and greater effectiveness" would be unlikely. And he was sure that such gradual progress was an essential feature in evolution, especially in the evolution of human beings and their high mental abilities.[154]

In conclusion, then, it can be seen that Hrdlička's legacy for the study of human evolution was a divided one. He was a strong, and in the U.S. a somewhat isolated voice in opposition to the reigning orthodoxy in the paleoanthropological world -- an orthodoxy whose main tenets were belief in parallel hominid phyla, acceptance of the abrupt displacement of the Neanderthals, and the tendency to put all of the major non- sapiens human fossils on side-branches of the human family tree. His critique of that orthodoxy was an incisive one, and he produced his own carefully worked out and boldly argued scenario of the later stages of human emergence; though that scenario did not change many minds when it was presented, it has managed to retain a certain degree of freshness while the ideas of his main opponents have largely faded. In addition, he brought to all his work on human evolution an interest in tracing patterns of adaptation that anticipated the concerns of modern physical anthropology.

On the other side of the coin, in his attempts to justify his own theories Hrdlička sometimes showed an excessive scepticism, and a hastiness to discount the antiquity, of fossils that seemed to defy his expectations. That he was close to being right in the case of Piltdown man should not make us forget his strained interpretations of Java, Peking and Rhodesian man. Though initially based on masterful analysis of supposedly ancient skeletons found in the Americas, his theory concerning the center of origin and pattern of migration of early Homo sapiens tended to interfere with his attaining an accurate reading of fossils found outside of Europe and its immediate environs. Finally, some of his methodological concerns, research interests, and notions about evolutionary processes were hide-bound, and probably did a great deal to mask the great virtues in his work noted above. Thus, while Ales Hrdlička's claim as institutional father of modern physical anthropology in the U.S. is a strong one,[155] intellectually he must be looked upon in the manner of a thoughtful, and interesting, uncle, who was perhaps born a bit too early to exert a strong influence on the younger generation.

CHAPTER IV

EARNEST A. HOOTON, 1887 - 1954

A General View of Hooton's Career and Influence

Earnest A. Hooton was one of the leading figures in twentieth century American anthropology. He was a prolific, and often entertaining, writer, an engaging public speaker, and an inspiring teacher. With the possible exception of Ales Hrdlicka, Hooton undoubtedly exerted a greater formative influence on the profession of physical anthropology in the United States than any other individual.

Hooton was born in Clemansville, Wisconsin on November 20, 1887. He attended high school in the city of Manitowoc, and took his undergraduate degree from Lawrence College. While a senior at Lawrence, Hooton qualified for a Rhodes scholarship; he did not travel immediately to England, however, but waited until after he had done graduate work in classics at the University of Wisconsin. In fact, both his M.A. and Ph.D. were in the field of classics and came from Wisconsin. Hooton later told his students that his original interest in anthropology had been kindled by a book he had read on the barbarian tribes of classical times, and that when he arrived at Oxford he had only decided to "take a flyer" in anthropology. The "flyer" obviously became a great deal more than that, for Hooton immersed himself in his new found field of interest while in England, under the tutelage of R.R. Marett (1866-1943) at Oxford and Arthur Keith (1866-1955) at the Royal College of Surgeons. Hooton took his Oxford diploma in the field of anthropology in 1912.[1]

In 1913 Hooton received an appointment as instructor in anthropology at Harvard University, where he remained for the rest of his career, eventually becoming one of Harvard's best known and best loved professors. When Hooton began at Harvard he felt qualified to teach either physical or what the British called "social" anthropology, but as the years went on he became concerned principally with physical anthropology.[2] Slowly expanding his own expertise and Harvard's course offerings in the subject, in 1921 he received an appointment as assistant professor. It was not until the early 1920's that Hooton accepted his first graduate student in physical anthropology, Harry L. Shapiro, who stayed on to earn his Ph.D. in 1926. Shapiro was the first in a long line of eminent physical anthropologists trained by Hooton; indeed, as Frank Spencer has pointed out, after World War II "the direction of the profession was determined almost wholly by Ph.D.s produced at Harvard under Hooton's direction" during the preceding two decades.[3]

In a recent retrospective look at Hooton's career Harry L. Shapiro sympathetically and aptly summarized Hooton's contribution to physical anthropology by pointing out the roles he played as teacher, popularizer and methodological innovator. As a teacher, Hooton not only attracted Ph.D. candidates to Harvard, but also helped spark a dramatic increase in undergraduate interest in anthropology there, a change which made the discipline much more visible than it had been, and provided a sizable crop of undergraduate majors who later went on to become professionals in the field. Hooton's reputation as a teacher, however, did not stem only from his stimulating performance as lecturer and classroom teacher, but also from his close personal involvement in his students' intellectual growth and professional progress. These qualities, when combined with the fact that he was for a long time the only full-time teacher of physical anthropology in the U.S., caused Shapiro to describe Hooton as the "father of the subject ... in an almost literal way."[4]

Hooton's role as a popularizer of physical anthropology grew out of his great success as a lecturer at Harvard. He began to receive invitations from Harvard clubs and professional societies to speak on physical anthropology and to illuminate public issues from an anthropological perspective. As his fame grew Hooton got the chance to address a wider range of forums and was often sought out by the media for "expert" comment on issues relating to his field. In addition Hooton employed his talents as a lucid and engaging prose sytlist to put out a stream of popular books and articles on human evolution and many other issues in biological anthropology, the most famous being his highly successful book on human evolution entitled Up From the Ape. [5]

As a methodological innovator Hooton's major contribution was to bring some of the methods of statistical analysis to the U.S. that the English biometricians had pioneered in the early years of this century. He applied statistical methods extensively in his anthropometric studies, and was the first anthropologist to make extensive use of IBM data processing equipment, in his last major research project on the population of modern Ireland.[6]

Though Hooton, as his synthetic and popular works showed, carefully followed new developments in all the major fields that impinge on physical anthropology, his own main areas of research were the skeletal biology of populations of the recent past and anthropometry. His most important research projects included an analysis of skeletal material from the aboriginal population of the Canary Islands; a study of a large sample of Indian skeletons unearthed at Pecos Pueblo, New Mexico; a large scale anthropometric study of the U.S. criminal population; and a comprehensive analysis of head form in the contemporary male population of Ireland. While in these studies Hooton did, as noted above, show himself to be a methodological innovator, he applied his methods in pursuit of what had been the traditional goal of physical anthropology for over 100 years -- the identification of racial and other sorts of physical "types," and the reconstruction of racial history by analyzing the proportions of various types in "mixed" populations. As has been pointed out by critics of this approach to physical anthropology, the function of quantitative analysis in such studies was generally to refine the definition of "types" that had already been discerned by the practiced eye of the anthropologist, rather than to approach the data in a fresh way.[7] The belief that these "types" were real entities rather than fortuitous combinations of genetically independent character traits was an _a priori_ assumption, usually based on the resemblance of some individuals in the population under study to individuals of presumed ancestral "types" or supposedly homogeneous "races" that had been studied before.[8] Indeed, the idea that the skeletal characters that were under analysis were overridingly determined by genetic rather than environmental factors was also an assumption, one without which the whole point of the exercise -- the "conjectural history" of racial "types" -- would not have been possible.[9]

As the critics have also noted, the "typological" approach to skeletal biology and anthropometry had a formidable staying power, and could weather adverse criticism even when it could not answer it. Hooton must bear a large share of responsibility for perpetuating this style of research, which tended for a long time to divert physical anthropology from more productive lines of research based on the analysis of function and adaptation. This is so both because of Hooton's great influence as a teacher, and because in his Pecos Pueblo study he developed a style of typological analysis that was widely imitated.[10] The increasing sophistication of Hooton's methods over the years was also a factor in what most recent students regard as the excessive longevity of the typological approach to physical anthropology, for that sophistication tended to mask the fundamental incompatibility of the theoretical assumptions that racial typologies relied on with the new-Darwinian "new synthesis"in evolutionary biology.[11]

As noted above, Hooton did not do much original work on fossil hominid morphology, or on primate comparative anatomy, which in that era were the central areas of research on human evolution. He published a few short articles on questions relating to human evolution in the early part of his career,[12] but did not take an active role in discussing the major controversies surrounding the subject until the mid-1920's. These years were a time of intense interest in human origins in the U.S., as we have seen.[13] The rise of Christian fundamentalism to political influence, as well as the public concern with racial and ethnic differences, seemed to draw biologists and anthropologists inevitably to the task of clarifying the picture of human evolution. Within the scientific disciplines which produced this picture, new pieces of evidence were continually being added, and their importance had to be elucidated and explained.

The opportunity thus existed for a fresh general synthesis of the problem of human evolution, one that would allow contrasting theories to qualify each other, and corroborating lines of evidence from different disciplines to make a combined impact. More than any other American of his generation, it was Hooton who saw this opportunity. He worked long and hard to take advantage of it, and achieved great popular success thereby.

In some ways, Hooton's eventual emergence as perhaps the most visible scientific figure writing on human evolution during the 1930's seems strange to the present day observer. His own original researches in physical anthropology dwelled almost exclusively on populations of anatomically modern Homo sapiens, and concerned themselves less with function and adaptation than with racial and typological analysis. Nor did he have the broad training in paleontology, genetics, and other branches of biological science that have become necessary for present day physical anthropologists working on human evolution. In other ways, though, Hooton was in a good position to take up the task he had set himself. First, his status as a non-specialist freed him to follow an interdisciplinary approach to the subject. Second, he could argue that he was not totally self-taught in evolutionary studies, since he had done comparative anatomy with Keith during his student days. Also, his position at Harvard gave him access to the expertise of specialists when he needed it. As the foremost teacher of physical anthropologists in the U.S., Hooton also had to keep up with current research on problems beyond those with which he was most directly concerned in his own research projects.

Most important, though, in explaining Hooton's attraction to, and success in, the role of synthesizer, are Hooton's own intellectual gifts. He was adept at characterizing controversial issues and theories in ways that revealed the critical pieces of evidence and assumptions around which they centered. As a writer he was fluent and entertaining; he delivered analysis and judgement with confidence and in readily quotable phrases. When no firm conclusion on an issue seemed possible, he confessed this candidly, and projected the image of an objective observer who was unwilling to force his evidence into a fit with preconceived theories. All in all, Hooton had all the tools required to become a respected authority vis à vis the general public.

While formidable, none of the foregoing qualities could substitute for original research in paleoanthropology in order to make Hooton a central figure in the development of scientific thought on human evolution. Still, "popular" work, especially in Hooton's case, has importance for several reasons. Hooton was a sensitive observer, and his synthetic writings provide a vantage point from which one can readily survey the state of evolutionary thinking in the period between 1925 and 1940. Also, as should be clear from the discussion of Osborn and others involved in the present study, what was said in popular articles took up a large part of the total scientific debate on human evolution in those years, and often conveyed an author's unique perspective concisely and directly. In a period in which there was only a handful of professional physical anthropologists in the English speaking world, Hooton's writings were a major element in the public's understanding of what the study of physical anthropology was, and what it could be expected to reveal about the nature and history of humanity. Finally, because of Hooton's importance as a teacher, his general writings can tell us something about the perspective he was trying to transmit to the next generation of professionals and the climate in which that generation was reared. Perhaps this perspective helped set the tone and agenda for Hooton's successors; if it has proved not to have had great influence, it might be interesting to consider why this has been so.

The Development of Hooton's Theoretical Perspective

From the discussion above, it might appear that Hooton's work ran on two entirely separate tracks -- i.e. professional research on modern *Homo sapiens* and popular writings on problems of human evolution. Of course the separation was not absolute; in fact, the groundwork for

Hooton's first large-scale foray into popular science, the book Up From the Ape,[14] was laid in three critical articles which appeared in more specialized journals between 1925 and 1930. Each article staked out his position on one of the then controversial issues surrounding human evolution. The first of them, a piece entitled "The Asymmetrical Character of Human Evolution," was meant to clarify the critical question of parallel phyla in hominid evolution, by means of a comparison of several fossil hominid skulls with those of modern apes and humans.[15]

As we have seen elsewhere,[16] in the mid-1920's a simple unilinear view of hominid evolution was almost universally in disfavor. Though people could agree on the existence of parallel phyla, they differed in their explanations of how these different branches of the human family had come to be established. Relative distances from the center of evolution, challenging versus unstimulating environments, and even descent from different pongid ancestors had all appeared as possible explanations. Hooton's article avoided the speculative high ground, however; rather than try to explain the phenomenon of hominid parallel phyla, he contented himself with the more modest task of documenting their existence quantitatively, and took as a given the position that multiple lines of evolution were to be expected as the result of normal evolutionary processes.

On the issue of parallel phyla, as on others relating to fossil hominids, Hooton showed himself to be following along lines laid down by his mentor, Sir Arthur Keith. At least as early as 1912, Keith had championed the view that "in the distant past there was not one kind, but a number of very different kinds of men in existence, all of which have become extinct except that branch which has given origin to modern man."[17] He had supported his position not only by promoting the authenticity and importance of the Piltdown fossils, but also by stressing the "specializations" of the supposed evolutionary dead-end represented by the Neanderthals, and by giving strong support to "fossils" like the Galley Hill skeleton whose antiquity was in doubt.[18] In the 1920's Keith spoke of the principle behind this pattern in fossil hominids as that of "discordant evolution" -- which in its simplest terms meant that "in some extinct races, while one part of the body has moved forwards another part has lagged behind."[19] As instances of such discordance he cited the perennial problem of the ape-like jaw and human-like skull cap of the first Piltdown skull, and also the supposedly human looking thigh bone found in association with what was then seen as the exceedingly primitive skull cap of "Pithecanthropus." Similar instances of mixed primitive and advanced characters, Keith argued, could be discerned if one studied the anatomy of living anthropoid species as well.[20]

Hooton's concept of "asymmetry" involved an idea very similar to Keith's "discordance," but he attempted to provide it with more rigorous and systematic support than Keith had apparently done up to that time. Working with casts of fossil hominid skulls and small samples of modern pongid and human skulls, Hooton selected 41 characters of the jaw and brain case and assigned six discrete states for each; a "1" denoted an "ultra-anthropoid" state for that character, a "6" an "ultra-human" one. The central results were reassuring to the theory -- each "type" revealed "asymmetry" by exhibiting a mixture of low, medium and high scores for different characters, especially the orang among the apes, and Piltdown among the fossil humans. The mean scores for each "type" also produced a predictable series -- with the male gorilla standing at 1.51, the "Alpine" Caucasian at 5.24, and the others ranged in between. The fossil hominids confirmed conventional "Keithian" expectations in their ascending order of humanness -- Java man, Heidelberg, Broken Hill (Rhodesian), Piltdown and Neanderthal (tied with identical 3.65s), Talgai, Combe Capelle. There was something for racists as well in the ratings of modern human groups, the order of composite scores from low to high being Eskimo, Australian, Negro, Mongoloid, Mediterranean, Nordic, and Alpine, with only the latter three attaining a composite mean in the "5" or "typically human" range.[21]

The neatness of these results, especially the last mentioned, raises obvious questions about the validity of Hooton's procedures. Several characters were surely selected for the very reason that "typical" Caucasians showed the greatest distance from gorillas in those features; and it was likely that his choice of "typical" members of other races was skewed in favor of specimens which seemed "primitive" in his eyes. While the purpose of his analysis was not to rank existing races in an invidious manner, the fact that the composite scores came out that way gives the whole procedure Hooton followed an air of spurious quantification -- numerical values were being pasted over what were in reality qualitative judgements about form. Confirming this impression is the fact that not all of the 41 characters chosen were ones that could be given exact measurements; even when measurements were made, results could be made to vary a great deal according to the place at which one drew the boundaries between a score of "5" and one of "6".[22]

The impression that theoretical assumptions were forcing particular results becomes stronger still when one looks at the conclusions about the path of human evolution that Hooton drew from his data. His major point was not that one could grade existing races on an ascending scale, or even so grade the known fossil "races;" rather, Hooton asserted, his data demonstrated that among modern forms of Homo sapiens the characters essential to the basic human mode of adaptation -- i.e. those connected with our large brain, erect posture and bipedal locomotion -- were fairly uniform. It was in relatively non-adaptive characters that great variation occurred, or as he phrased it, "nature evidently likes to proceed to extremes in non-essentials."[23]

The evolutionary scenario that Hooton felt his data supported was the following one:

> most evolutionary asymmetries are to be ascribed to the fact that several distinct stocks whose common ancestry must be sought in a proto-human or very inferior human stage of development have developed along lines roughly parallel, but with many unimportant divergences. Not all of these stocks attained to precisely the same status of evolutionary development as measured by the degree of their departure from characteristically anthropoid forms. Some of these became extinct without having realized essentially human levels of development; others have survived at varying stages of morphological evolution, the asymmetry of their development being manifested far more in the mosaic of primitive and highly evolved characters within the same type than by average development of the sum total of their bodily characters.[24]

For him, the main exphasis was thus on multilinearily and "asymmetry," rather than on the different stages of development represented in each "type," though the existence of such stages was recognized in a general way, and easy to explore further, should one choose to glance at the numbers he provided.

Hooton wanted to be sure that he was not seen as preaching an extreme brand of polyphyletism. The tropical forests of the Tertiary epoch, he asserted, had not "rained anthropoids, many of which evolved into men." He also expressed the view that the racial variety of present day humanity was probably the result of hybridization between only "two or three primary types."[25] Indeed, similar episodes of hybridization might have occurred in the earlier stages of hominid evolution, even when adjacent forms might technically have belonged to different species.

Though such a situation would seem highly unlikely according to the modern concept of speciation, Hooton was writing before this aspect of the "new synthesis" had gained wide currency. Chimpanzee-gorilla hybrids, he noted, had been reported and were, in his view, "not wholly incredible." Since human ancestors had probably shown "superior adaptability, greater initiative and less conservatism" then their pongid relatives in other aspects of behavior why not assume that they had revealed these qualities in the area of mating habits as well?[26] While it rings oddly today, Hooton's attribution of behavior characteristics of neighboring conspecific populations to distinct species or even genera (as most forms of fossil humans were given generic status at the time) was not uncommon in the early part of the century.[27] The truly distinctive feature in the picture above was rather Hooton's stress on the superior "initiative" of human ancestors vis à vis their close relatives. As we shall see, it remained a keynote of his whole conception of the problem of human evolution.

At the close of his article Hooton broached another theme that would also carry through into his later work, a theme that he saw as closely related to "asymmetry" in evolution. This was the idea that there might have been multiple centers of hominid emergence and evolution as well as multiple lines of descent. His remarks on the issue were brief -- he noted that since both Africa and southern and southeastern Asia seemed to have been home to both fossil and recent anthropoids, and since both seemed to have possessed environments in which hominids might have evolved, it was not reasonable to rule out either region, or to seek some less likely spot as a single center of hominid evolution. Even if the first "arboreal ancestor" of humankind distinct from the anthropoid stock had first appeared in only one of these regions, he believed there was no reason why members of that species could not have spread across the whole range of environments favorable to its further development quite early in its evolutionary career.[28]

The idea of multiple centers of human evolution was a controversial point in the mid-1920's, the heyday of the central Asian theory, and Hooton chose to return to it at greater length in 1927, addressing an audience of both professionals and educated laypersons in the British journal Antiquity.[29] Again he intertwined the idea with those of multilinearity and "asymmetry." Was evolution, he asked,[30] to be conceived as

> an essentially unilinear process, operative exclusively in a single area or in a few areas, or is it rather to be regarded as a universal process which works continuously, but variously, sometimes rapidly, sometimes slowly, upon all organisms at all times, in whatever environment they find themselves?

For his part, Hooton preferred the second model. In his view the "asymmetries" among the oldest fossil humans -- those of Java, Piltdown, Heidelberg, and Rhodesia -- made it "hardly conceivable that any one of them could be a direct ascendant or descendant of any other."[31] The most plausible explanation of these differences was that Nature had made "a number of experiments in developing anthropoid forms in a humanoid direction."[32] Once admitting multilinearity, he asked, did it make sense to set aside a single region as the sole laboratory for Nature's experiments? The dryopithecines, which, in agreement with Gregory, Hooton saw the most likely group in which to find man's last arboreal ancestor, had been widely distributed over southern and southeastern Asia, over Africa, and even over parts of Europe in the Miocene epoch. Why, he wondered, should not progressive, generalized members of this stock not have spread to take advantage of the terrestial bipedal mode of adaptation wherever environmental opportunities for it had existed? Hooton made clear to his readers that he was not echoing what was to him the tenuous theory of Hermann Klaatsch (1863-1916), which derived living races of mankind from different anthropoid genera. His "progressive, generalized" dryopithecines would, he asserted, have stemmed from one or a limited number of species, and would have resembled each other more than they resembled the ancestors of any living apes; also many of these "early and crude attempts" in the "humanoid" direction would have become extinct and would have made no genetic contribution to modern races of humans.[33]

Hooton was able to make a good case that the fossils, as they were then interpreted by the majority of experts, were on his side. His portrayal of the phenomena of multilinearity and asymmetry were well adapted to an era of vigorous "splitting" in paleoanthropology, an era in which discoverers and describers emphasized the uniqueness of their finds and liked to place them in distinct genera. It was also a way in which the momentous obstacle presented by the Piltdown specimens could be surmounted without questioning their importance or that of the other major fossils then known. Still, Hooton also had to comment on competing theories of hominid origins and dispersal. Specifically, what did he have to say about the central Asian theory, and the pattern of mammalian evolution and dispersal that had been first propounded by Osborn and Matthew, and applied to man by Osborn, Black, et al ?

For Hooton, the Osborn-Matthew pattern of concentric rings of distribution of related mammalian forms, with the most primitive form at the periphery, and the most advanced at the center of dispersal, seemed to constrict the evolutionary process unduly. First, it appeared to him to assume that "environments inevitably migrate and that more conservative mammals follow those environments," without really showing that such mammals could not remain were they had first appeared, and in that place either evolve further or become extinct. Second, and more important, it seemed to imply that "evolutionary forces operate only on animals which have remained at the spot in which they originated," and could not affect "primitive" forms living far from the center of dispersal. When combined, Hooton argued, these notions forced the conclusion that "the places where one finds primitive existing forms of any order of animal are exactly the places where these animals could not have originated." Pushed to what he called their "logical extreme" the theory would "lead us to look for the birthplace of man in that area where there are no traces of ancient man and none of any of his primate precursors."[34] Though this attempt at a reductio ad absurdum might seem strained, Hooton could point to an authentic example of such reasoning in the writings of Davidson Black, who proposed to look for the center of human dispersal in a region that had produced neither hominid nor anthropoid fossils at the time Black began his search.[35]

Beneath these issues, though, there seemed to be a more fundamental problem for Hooton with the Osborn-Matthew theory, one that went to the very heart of what Hooton conceived to be the difference between "progressive" and "conservative" animals. If a "conservative" species were one which had great difficulty adapting to change, would it not have a tendency to succumb, and "die hard in the home ditch" rather than adapt or migrate? Conversely, he asked, wouldn't "progressive" and adaptable species be "those who move into new environments rather than those who wait for new environments to move into them?"[36] Again, as in the discussion of asymmetry in evolution, the question of "initiative" had arisen. The terms "progressive" and "conservative" seemed to have a moral and psychological dimension for Hooton, and not just a biological one; the contrast was not just between species that evolved in a certain direction and those that did not, but also between those that put themselves into new evolutionary situations and those that did not. Seeing evolution as a process in which animals were passively molded by their environment thus seemed myopic to him; for higher mammals, and for higher primates especially, some kind of behavioral pre-adaptation towards progressive evolution, some sort of in-built opportunism, seemed a necessary element in the evolutionary drama.

What the nature of the "proto-hominid's" behavioral contribution to his own evolution was exactly, was an issue about which Hooton as yet did not have a developed theory. He did however, register further his lack of enthusuasm for mechanistic theories of primate evolution in an article which appeared in the recently founded journal Human Biology, in 1930.[37] In that article

Hooton developed the view that then reigning functional explanations of primate phylogeny, though plausible, either required questionable subsidiary principles (e.g. Lamarckian use inheritance) or failed to account for anomalies in the body of evidence they purportedly based themselves upon.

Hooton aimed his first salvo at the most illustrious target -- the "arboreal theory" of Sir Grafton Elliot Smith, especially as it had been developed by Smith's pupil Frederic Wood Jones. Briefly, the theory asserted that increasing adaptation to the special conditions imposed by arboreal life in a tropical region could explain the unique set of features in the brain, skull, posture and forelimbs that set higher primates off from other orders of mammals. The supreme emphasis that arboreal life had placed on complex interactions between the eyes, brain and forelimbs had resulted, according to Elliot Smith, in a series of critical functional and morphological changes as the primates developed -- e.g. increasing dominance of the visual over the other sensory areas of the brain, the reduction of the snout and the forward placement of the orbits, stereoscopic vision, and "emancipation" of the forelimbs from exclusively locomotor function. This series of arboreal adaptations, Elliot Smith had also argued, constituted an indispensable set of preadaptations for the emergence of bipedalism, extreme manual dexterity, and the highly developed cerebral cortex in human beings.[38]

While praising the arboreal theory for its elegance and ingenuity Hooton detected several problems in the way in which it was usually formulated, problems which were unsettling to him. First, he noted, both Elliot Smith and Wood Jones had portrayed the small nocturnal primates of the genus _Tarsius_ as modern survivors of what had probably been the "jumping off point" from which the higher primate series had developed. If this were so, Hooton wondered, why had these "structural ancestors" of later primates ceased to evolve within their arboreal environment? Why hadn't the challenge of arboreal life continued to push them beyond a primitive stage of development in brain, vision, and manual dexterity? Also, Hooton noted, the theory seemed to neglect the fact that a major group of arboreal primates, the lemurs, did not show the quintessential "arboreal" features of emancipation of the forelimbs from locomotion, regression of the snout, or forward displacement of the orbits. It seemed to him, then, that some other preadaptations, or environmental pressures, were necessary to account for progressive evolution in the direction that the arboreal theory described.[39]

Hooton had some other quibbles on matters of detail, but his other major objection to the theory was philosophical. The arboreal theory seemed to him to be based on a Lamarckian assumption -- it appeared to imply that if animals exerted themselves in a certain direction, eventually heritable organic change making those exertions more efficient would occur. Unfortunately, he pointed out, this sort of process had never been documented in empirical studies of animal inheritance.[40] Hooton was probably not being unfair, since the language used by Elliot Smith and others could have been seen as implying use inheritance and evolution by exertion;[41] it is important to note, though, that the theory itself could be phrased in rigorous selectionist terms and still retain its essential meaning. What Elliot Smith was talking about could have been accomplished through a series of interacting "feedback loops" involving the hands, eyes, brain, and locomotor mechanisms and the genes that helped determine their respective forms; mutations leading to greater efficiency in any of these loops would have been preserved by natural selection. Thus, what was Hooton's strongest "suspicion" about the arboreal theory did not count against it as much as his other objections.

A "functional theory" that Hooton could dispose of much more easily was the old idea that the reduction of the snout in the hominid line leading to man made the growth of man's large brain possible by decreasing the pressure of the temporal muscles on the skull vault during ontogeny. If such a mechanical principle were valid, Hooton argued, one would expect it to apply throughout the primate order; yet, in neither the Old World nor New World monkeys did the expected negative correlation between the degree of development of the jaws and face versus that of the brain appear, nor did large "snout" size or small brain size in these animals vary directly with the degree of development of the temporal muscles. Among anthropoids, noted Hooton, the gibbon appeared to have a large facial region for its body size, yet it possessed neither exaggerated temporal muscles, nor the cranial superstructures which betokened large muscular pressures on the skull vault. Finally, he contended that ontogenetic studies had shown the principal growth of the brain to be substantially complete before growth of the jaws and chewing muscles had reached its peak. Reduction of the face and expansion of the brain might indeed be key differences between man and ape, but a simple functional and mechanical explanation of the changes was insufficient.[42]

Another theory dealing with the evolutionary relationship between man and the great apes that Hooton had qualms about was Keith's idea of a brachiating stage in man's ancestry. Here the quarrel was not so much with Keith's hypothesis as the uses to which it had allegedly been put by others. Hooton did not doubt that progression by means of arm swinging and the adoption of erect sitting in the trees were the product of crucial adaptations in the arm, shoulder, pelvis, back, and foot that also formed part of the human anatomical heritage. Still, he asserted, one had only to look at the variety in the modes of progression that modern apes adopted when on the ground in order to realize that other changes had to have occurred beyond those seen in known "brachiators" in order to make an incipient hominid pattern of ground progression efficient. What those changes had been might be partially deduced through further studies of primate comparative anatomy, but how and why they had occurred had not yet been sufficiently analyzed. The brachiating "stage," he concluded, was a preadaptation for such changes, but most of them had probably occurred "subsequent to the habit of erect ground walking" presumably attained by the earliest proto-humans, by a process little known as yet.[43]

From Hooton's point of view, anthroppologists had been overly optimistic about the explanatory power of functional theories regarding the differentiation among the various human races, as well as regarding the evolutionary transition between apes and humans. For example, he noted, a functional correlation was often made between dolicocephaly and hypertrophy of the jaws by considering these skeletal features as jointly resulting from muscular stresses associated with a rough diet. Yet, Hooton asserted, when one looked at the Eskimos -- long a favorite example of these theories -- one found their diet not to be as difficult to chew as was generally believed, and hypertrophy of the jaws and dolicocephaly to be largely male characters alone. Also, he claimed that there was a gradient in the extent of narrowheadness among Eskimo populations as one went from west to east, a change that was inexplicable by the terms of the functional theory. Furthermore, Hooton doubted whether the basic assumption involved -- that the temporal muscles pressed inward on the skull during mastication -- had ever been established, experimentally or otherwise. Similar objections, he thought, could be raised against other racial characters with supposedly functional value -- e.g. steatopygia as a means of energy storage, the supposed correlation between nasal breadth and climate, or between the latter and skin color. The idea that most such characters were heritable, relatively stable under diverse environmental conditions, and of limited adaptive significance seemed more reasonable to Hooton.[44]

Hooton's attachment to this conclusion was not surprising, of course, since it rendered the racial analysis of "mixed" populations much simpler by increasing the number of stable characters that would follow a race through its various migrations in space and time.

For Hooton, all of the foregoing problems justified a state of mild scepticism about the cumulative results of the study of human evolution to date: though scientists were beginning to know something about the stages of human evolution, they were in his view "quite unable" to identify its causes.[45] On the issue of how major organic change in general occurred, he saw a similar impasse:[46]

> Evolution by response of the organism to its environment and by hereditary perpetuation of such responses does not accord with experimental data; evolution by chance selection of combinations of characters -- all inherent in the original germ plasm -- puts more strain on the credulity than all of the fantasies of primitive cosmology.

Though Hooton knew he had to be careful about Lamaarckian interpretations and had criticized their use in the "arboreal theory," his conviction that hominids had somehow played an active role in their own evolution pushed him very close to such a formulation in the end. "One would like," he concluded, "to think of human evolution as a process of age-long striving of animals gifted with a divine spark of initiative, fanned into flame by favoring winds and by a sort of 'spontaneous combustion.'"[47] Again, Hooton had made reference to that mysterious quality of "initiative" as a major element in the evolution of humankind.

If theories in evolutionary science often do have within them echoes of reigning social values,[48] one would have to characterize this view as the "executive" analogy -- i.e. that the firm of man has reached its present state of profitability through the contributions of a long line of hard-driving employees, with the product managed being the managers themselves. The brand of "progressiveness" that Hooton was invoking in human emergence was a clear analogue to the business-oriented version of "progressivism" abroad in Herbert Hoover's America. The personal quality of "initiative" that was so commonly invoked in the success literature of the "New Era" had become critical to evolutionary success as well.

Hooton's First Synthesis -- Up From the Ape

These articles, while interesting in themselves, were perhaps more significant in their role as preparation for Hooton's best known book, Up From the Ape,[49] which appeared in 1931. This work represented Hooton's attempt to draw together all the scattered evidence and theory bearing on human evolution that he had been grappling with in his articles and his courses at Harvard. While there was much that seemed new in the book compared to his previous writings, its central themes were those Hooton had already begun to enunciate between 1925 and 1930 -- the importance of multilinearity and asymmetry in human evolution, the significance of non-adaptive characters as marks of genetic relationship, distrust of functional and mechanical theories of evolution, and the belief that hominids had been "creatures of destiny" who had contributed to their own progressive evolution. As was true of the earlier articles also, the last two ideas were probably those of greatest significance in defining Hooton's personal attitude toward the problem of human emergence, while the first two embodied his own way of crystallizing the general climate of opinion regarding fossil humans.

The first few chapters of Up From the Ape revealed Hooton's personal attitudes particularly well. Throughout his account of the various functional characteristics which distinguished the human adaptive pattern from those of the other primates, he showed himself to be very much in the school of Keith and Gregory in stressing the qualitative similarities between pongids and humans in form and behavior; he also made clear his belief that the pongid stage had been a necessary preadaptation for the human one. Still, whenever the crucial issue of degree of intelligence, and the related ones of problem-solving, tool use and speech arose, he insisted on placing a great quantitative gulf between humankind and its anthropoid relatives. Yes, Hooton conceded, man probably was descended, as Gregory theorized, from a dryopithecine which crossed the pongid-hominid threshold during the Miocene; yet the idea that the first hominid could have survived on the ground with a level of intelligence comparable to that of the present day "conservative" apes seemed to him absurd.[50] He also recognized the importance of the findings of R.M. Yerkes (1876-1956) and Wolfgang Köhler (1887-1967) on the problem-solving and tool using capacities of the great apes,[51] but he stressed his belief that their overall ability to "profit by experience" was distinctly limited -- far more limited than their ability to manipulate objects.[52] The notion that primitive tool use, on the order of that exhibited by present day apes, could have provided enough raw material in the earliest "proto-humans" for the further evolution of intelligence toward human levels -- either through use-inheritance or natural selection -- Hooton also rejected. "If our ground dwelling forebears," he contended, "had not been more intelligent than the chimpanzees of today, nothing they would have done with their hands would have increased the size of their brains."[53]

As with the hands and tool use, so also with the important issue of the relationship between the vocal tract and articulate speech. While Hooton admitted that reduction of the jaws and the greater flexure of the cranial base in humans versus the great apes had made speech sounds easier to produce, he still maintained that the key difference was in the brain. "There is," he asserted, "nothing about a snout (a colorful term whose use Hooton recognized was imprecise) which prevents its possessor from speaking, but there is something about the brain that goes with a snout which makes speech impossible."[54]

Hooton also refused to credit one of the mechanistic theories which supported the view that the first hominids had approximated the anthropoid stock closely in both brain and body -- namely the hypothesis that mere environmental change, in the form of dessication of forest habitats in the Miocene era, had forced both the development of bipedalism and the emergence of human hunting and tool using behavior. No, he replied, in an apparent contradiction of his earlier critique of the

Osborn-Matthew theory, "when the climate changes most animals follow their congenial environment."[55] Protohominids had been able to cash in on the opportunities provided by ground dwelling, despite its great risk of increased predation, because they had started at a level higher than the "Tory"-like anthropoids of today.

In Hooton's view only a "progressive animal" could try "to shift his habitat to a more favorable environment."[56] "These radical ancestors of ours," he eulogized,[57]

> saw and acccepted the chance of a larger, more varied, and fuller diet; they wanted to live their lives more abundantly. A careful and dispassionate examination of the facts and probabilities of human evolution indicates that this crucial event [the adoption of ground dwelling] was not the result of an environmental accident, but rather the manifestation of that superior intelligence and initiative which, inherent in the proto-human stock, determined its evolutionary destiny.

It is interesting to note how Hooton tried to support what one might call his "frontier" of "free-enterprise" theory of human emergence by stretching both anthropological and paleontological evidence to fit his notion that huge gaps must have existed between ape and "proto-human" levels of intelligence. Examine, he said, the triad chimpanzee - aboriginal Australian - Englishman. "The native Australian," he contended, "is almost as incapable of absorbing [European] civilization as the chimp of adopting the method of life and tribal customs of the aboriginal Australian. Yet the native Australian is a human being and behaves with insight" in a way that the chimpanzee can not approximate. All three creatures had had equal amounts of geological time to develop civilization, he asserted, but biological endowments had fixed their relative levels of accomplishment. A similar distinction in levels of inherent capacity for culture existed, he said, in a single environment -- the tropical forests of Africa -- among the Bantu speaking Negro, the pygmy, and the lowland gorilla.[58]

In his discussion of fossil hominids Hooton used "Pithecanthropus" to make a similar point. Though the fossil was then generally assigned a late-Pliocene or early Pleistocene date, Hooton, like others at the time, preferred to see Java man as a "late survival of a conservative humanoid stock" stuck, as it were, on a very low level of intelligence and cultural sophistication. Thus, he could then point to its relatively large brain -- intermediate between the gorilla and Homo sapiens in both size and convolutional complexity according to the American neuroanatomist Frederick Tilney (1875-1938) -- in order to underline the "fact" that hominids had been far above the apes, "erect in posture and dominant in brain" even in the "Tertiary forests" of the pre-human dawn.[59]

As with the evolutionary transition between pongids and hominids, so also with the more general theory of primate brain evolution advanced by Elliot Smith. Hooton again felt a need to register his doubts about what he saw to be facile functional explanations. The specific criticisms on matters of detail were the same as in the article of the previous year, while the attack on Elliot Smith's general logic was amplified somewhat.

Thus Hooton characterized Elliot Smith's picture of mutual evolutionary interaction among brain, eyes and hands as a "vicious circle of reasoning." Of course, increased intelligence made more complex hand and eye coordination possible, and a larger brain would produce the requisite increase in intelligence. But to say that more activity involving hand-eye coordination caused brain growth and that brain growth caused more hand-eye coordination seemed to Hooton to reduce to the vacuous proposition that "man owes the large size of his brain to his intelligence, and owes his intelligence to the large size of his brain."[60] As has been noted above, the mechanism by which these changes might have occurred was not entirely clear in Elliot Smith's theory, but that one could not be supplied was due not to a defect in the latter's logic, but to Hooton's reluctance to credit the most likely alternative -- the mechanism of chance variation and natural selection.

Hooton's awe of the "superior intelligence and initiative" that marked the human stock, and his distrust of mechanical explanations for their appearance, was so great that he came close to following in the footsteps of Alfred Russell Wallace, Darwin's contemporary, by attributing these qualities to some sort of "design" in nature. This idea surfaced indirectly in Up From the Ape, during a discussion of the American biologist Raymond Pearl's (1879-1940) analysis of the most common causes of death in contemporary human beings. Pearl had broken down mortality statistics according to the various organ systems whose failure had been the main cause of death, and then traced each organ system to the embryonic layer from which it had originally differentiated. Pearl had concluded that the entoderm, the source of the alimentary and respiratory systems, was responsible for 57% of human deaths, while the ectoderm, the germ of the skin and the nervous system, was only responsible for 8 to 13%. Pearl had then hypothesized that the divergence had resulted from differences in the extent of each layer's evolution from the primitive vertebrate condition -- in his view the ectoderm was the most evolved and the entoderm the least evolved. To him this had constituted a fine illustration of the opportunism of natural selection -- though imperfect, the comparatively less evolved entoderm had continued to be adequate to the task of survival and reproduction, while the ectoderm had had to become a "better product" in order to accomplish the same goal in a creature with the complex nervious system of man.[61]

For our purposes, whether Pearl's theory was convincing is immaterial; what Hooton made of it is interesting. Hooton was not sympathetic, echoing Wallace in objecting that the mechanism of natural selection was insufficient to explain the perfection of the human nervous system. "We have much larger and better brains," he contended, "than we need for the rough and tumble of natural selection." If natural selection alone had ruled over the evolution of the brain, it like the alimentary or respiratory system would have had the "least efficiency and durability compatible with survival, and all organs being reduced to the same level of mediocrity, we should all go to pieces at once like the one-hoss shay." Perfection seemed to imply more than mere chance, and indeed, so did imperfection, for according to Hooton there was no necessary connection between it and randomness. "Imperfection," he asserted, "rather implies intelligence in design -- a limited intelligence however."[62] In the matter of rejecting the efficacy of natural selection Hooton evidently wanted to have it both ways, even at the cost of logical consistency. However, though he made his distrust of mechanism clear, he failed to go further and make the nature of his vitalistic or supernatural commitments explicit. His readers had to suffice with the following cryptic formulation: "we need not give man and his ancestors the credit of developing their own intelligences, but if a human being is not a manifestation of an intelligent design, there is no such thing as intelligence."[63]

Though he did not attempt to clarify his overall conception of the respective roles of chance and design in evolution, Hooton did return to the issue of how evolutionary change took place, and tried to develop his own brand of compromise between selectionism and Lamarckianism. The latter position, he noted, could be represented by two variants -- first, that the environment somehow molds variations which then pass into the "germ plasm" and become heritable, and second, that somehow organisms can transmit adaptations that they have developed by "striving" to adjust to their environments. Very much like Hrdlička,[64] Hooton felt that the key to the question of whether Darwinian or Lamarckian processes were most important was the complexity of the evolving organism in neural organization. On the lower levels of nervous complexity, he believed, organisms were passive in their own evolution, and selection, along with the modifying force of the environment, held sway. On the highest levels, however, organisms could choose and even change their environments, and thus had a great measure of control over "the direction of their own bodily adaptation."[65]

With this formulation, Hooton seemed to forsake his previous caution in order to put forward an unabashedly Lamarckian explanation of human evolution, for humans clearly represented the highest level of nervous complexity that existed in nature. One of the great benefits of Hooton's brand of "compromise" was its inbuilt resistance to disproof -- the fact that laboratory genetics had uncovered no evidence of the organism's ability to control the direction of its own "bodily adaptations" was only to be expected, given the lowly nature of the experimental material involved. In addition, the theory seemed to imply that further human evolution would have to be largely self-directed and Lamarckian in nature, since the principal product of the human nervous system, that behavioral capacity which Hooton at one point called "intelligence <u>or</u> initiative [emphasis mine]" had become the primary cause of continuing human evolution.[66]

As might be expected in a work that spoke so much about superior intelligence as the leading factor in human evolution, Up From the Ape, when it dwelled on the "stages" rather than the "causes" of evolution, attempted to present an up to date view of the development of the human brain, from both ontogentic and phylogenetic perspectives. In his phylogenetic analysis of the brain, Hooton tended to follow Anglo-American authorities -- principally Keith, Elliot Smith, Wood Jones and Frederick Tilney. There was little that was new in his comparative anatomical account of brain evolution. The subtleties of the "encephalization" idea that had been elaborated on the continent by Eugene Dubois (1958-1940) and others were hardly mentioned. Instead, Hooton cited simple arithmetical ratios on the relative proportions of brain and body in humans versus the apes in order to demonstrate human superiority.[67]

Regarding the issue of cerebral localization of psychological functions, Hooton, as was customary at the time, stressed the special role of the frontal association areas in allowing the elaboration of typically human forms of behavior such as our "higher ideals of conduct." But following Elliot Smith, he reminded his readers that it was really the cooperation of all the areas in the "neopallium," and the association areas particularly that made truly human behavior possible. He also explicitly distanced himself from what he considered to be the extreme versions of cortical localization theory propounded by some European neuroanatomists.[68] Similarly, in looking for cortical evidence of speech capacities, Hooton noted the importance of the so-called "Broca's area" (the third inferior frontal convolution) in actual speech production, but again cited Elliot Smith in stressing that human language required a complex "central exchange" composed of this area and several others; only then could words be endowed with meaning as well as reference to specific situations.[69]

Like other theorists of the pre-World War II era, Hooton attempted to fill out his discussion of brain evolution with qualitative analysis of fossil endocranial casts. He also followed standard practice by paying lip service to the warnings of James Symington,[70] while proceeding at the same time to extract from the casts all the details of form he needed, just as though the casts were the brains themselves. Hooton justified his uncritical reading of details from the endocranial casts with the following seemingly cautious statement: though correspondence between skull vault and brain was inexact, "a number of ridges and depressions of the skull walls do correspond with and define brain areas, so that the cast not only outlines the general shape and proportions of the brain but even permits the anatomist to distinguish some few blurred details of pattern."[71]

As in method, so also in results Hooton had no real surprises to offer. For his interpretation of "Eoanthropus" he followed Keith's second reconstruction of the Piltdown skull; for the rest of the major fossils he used Frederick Tilney's 1927 work, The Brain From Ape To Man as his basic source of data.[72] The ultimate findings were thus predictable: the principal fossils appeared to show a morphological sequence of ascending complexity that matched their assumed positions in human phylogeny -- the actual order being "Pithecanthropus," "Eoanthropus," Neanderthal man (with Rhodesian man as an inferior sort of Neanderthal), and Upper Paleolithic *Homo sapiens*. The key features justifying that sequence were the cortical speech areas and the association areas, with special attention paid to the frontal lobe. According to Hooton, even the most "primitive" hominid brain, that of "Pithecanthropus," appeared to have had the physical basis for rudimentary speech abilities.[73] The next in the series, "Eoanthropus," was alleged to have possessed a third frontal convolution (Broca's area) of "essentially human proportions, little if any inferior to that of a native Australian of today."[74] Neanderthal man, Hooton asserted, had had a third frontal convolution and an "auditory eminence" on its temporal lobe (an area implicated in speech perception) more developed than those in both the Piltdown and Java fossils.[75] The brain cast of Rhodesian man supposedly showed marked similarities to the Neanderthals, but with less complex frontal convolutions and a "more simian" form of the temporal lobe.[76]

According to Hooton, analysis of the association areas generally revealed the same morphological and psychological sequence as did that of the speech areas alone. Neanderthal man, for example, showed association areas greater in extent than his predecessors, with the significant partial exception being that the frontal lobe was "proportionately less expanded than the other parts of the cerebrum." In order to explain this "fact" he repeated Tilney's interpretation without qualification -- i.e. that the highly developed frontal association areas were the "latest acquisition of human brain specialization." Of course, this was not to say that the frontal areas were the only ones in which _Homo sapiens_ was superior to his brutish cousin the Neanderthal; Hooton pointed out that in the other association areas, and especially in those related to speech, the Neanderthal endocranial casts were less developed than those of Upper Paleolithic Homo sapiens fossils.[77]

Hooton thus saw the evidence of "fossil brains" as confirming that of comparative anatomy and psychology -- i.e. that even the oldest and most primitive hominids so far discovered had had a great superiority over the apes in the crucial area of intelligence, and that this superiority went far back in evolutionary time. Interestingly, the two fossils discovered during the 1920s which might have called these conclusions into question, Australopithecus africanus and "Sinanthropus," Hooton did not analyze. "Sinanthropus," which would eventually cast doubt upon the notion that Java man was merely a late surviving remnant of a possible Tertiary human ancestor, had only been recently discovered; in addition, the first relatively complete skull of Peking man had not had its endocranial cast described in detail at the time Hooton's book came out.[78] The Australopithecus endocranial cast, however, had been interpreted, and what one might call its "supra-anthropoid" characters had been stressed by Dart, but Hooton had the option of ignoring it, since the early weight of opinion had refused to grant the Taungs fossil hominid status.[79] That Hooton was similarly inclined can be inferred from his single reference to Australopithecus in the book, where he described it as an African fossil "alleged to be a humanoid ape of the Pliocene."[80]

There was more to fossil hominids than their endocranial casts, and Hooton's discussion of the fossil record did not neglect other parts of the skeleton. As with the discussion of the brain there was little novelty in the details of his analysis; his major phylogenetic findings, while more extensively stated, essentially duplicated those contained in his previous writings -- asymmetry and multilinearity remained the keynotes of hominid history.[81] As far as the individual lines of evolution were concerned, he argued that of the various "progressive dryopithecine" stocks which had emerged during the later Miocene, the most primitive lines, leading to "Pithecanthropus erectus" and "Sinanthropus pekinensis" respectively, had probably resulted in the evolutionary dead end of extinction.[82] The "Neanderthaloid" line, which, Hooton noted, had "probably evolved to a low human status in the Pliocene," had probably met its end during the last glacial epoch, when it had been replaced by intellectually superior representatives of Homo sapiens.[83]

Hooton's sympathy for English theorists, and Keith especially, showed in his portrayal of the allegedly "early Pleistocene" Piltdown skull; he hypothesized that Piltdown probably represented the precursor of the supposedly early _Homo sapiens_ Galley Hill skeleton, and thus constituted a type more closely approximate to "the ancestral form of modern Europeans than any prototype yet discovered."[84] Whether it was actually ancestral to _Homo sapiens_ Hooton left open. Like most of the parallel phyla theorists whom Hrdlicka criticized, Hooton's multilinear scheme left him without a fossil that he could claim as a direct ancestor of Upper Paleolithic _Homo sapiens_ (except for the geologically questionable Galley Hill fossil).[85]

While Hooton's depiction of the human family tree was similar to most others of the time, it still had points of emphasis that set it apart from some that we have encountered earlier. Most important were the areas where Hooton moderated his views so as to distinguish them from extreme believers in multilinearity and parallel evolution such as Osborn. Thus, while he talked about the gulf in brain and mind betwen apes and humans, he refused to give credence to the "Pro-Dawn man" theories of Osborn. Not only did humans and great apes share a common anthropoid ancestor after the brachiators had diverged from the rest of the primate stock, asserted Hooton, but the split between apes and humans had come after that common ancestral line had achieved large body size. The idea that the hominid stock had contained "homunculi" as early as the Oligocene seemed extremely dubious to him, for such creatures would have been at a severe disadvantage in the struggle for existence as ground dwellers.[86]

Hooton also differed from Osborn in his characterization of the crucial problem of the replacement of the Neanderthals by Upper Paleolithic Homo sapiens. To a degree about midway between those exhibited by Hrdlička and MacCurdy respectively Hooton downplayed the supposed inferiority of the Neanderthal population; instead, he granted them "considerable manual precision and some ingenuity," and noted that they had probably had ideas about death, a fact which in his view indicated that they deserved "full human rank."[87] On the other side of the issue he questioned the glowing picture of the "Cro-Magnon race" put forward by the French, to which Osborn had given so much attention in Men of the Old Stone Age.[88] Not only had the average brain size of the Cro-Magnon people been overestimated, argued Hooton, but also the quality of their art and its usefulness as an indicator of superior intelligence.[89]

Indeed, Hooton objected to the idea that the "Cro-Magnons" were a "race" at all, if one employed as one's criterion the existence of a group of "heritable, non-adaptive features" in which the population under consideration showed "a certain homogeneity." In surveying all of the European Upper Paleolithic skeletons, he could not find a single feature in "Cro-Magnon" man which did not have a wide distribution among non-"Cro-Magnon" fossils; also, most of the features that others singled out he regarded as of doubtful value as racial indicators, since the "Cro-Magnon" skeletons themselves were hardly homogeneous in them.[90] In fact, asserted Hooton, the unique morphology of the skeleton most often seen as the "type" specimen of this race, the so-called "old-man" of Cro-Magnon, could be best explained as resulting from hybridization between two other Upper Paleolithic groups, an "Ofnet brachycephalic strain" and a "predominatingly dolicocephalic stock of the Galley Hill type."[91]

Though he avoided some of Osborn's extravagance regarding Cro-Magnon man, Hooton managed to paint himself into other theoretical corners regarding the Neanderthal - Upper Paleolithic transition. The previous hypothesis about Upper Paleolithic "hybridizations" brings up one of the central ways in which he did so, for as in his work on modern races so also in his discussion of fossil races Hooton was willing to entertain the possibility that racial mixing had occurred upon faint morphological and no historical evidence of actual contact. For example he alleged that Rhodesian man and Neanderthal man, after periods of parallel evolution *vis à vis* the line leading to *Homo sapiens* extending back into the Pliocene, could have made genetic contributions to modern human populations. The Neanderthals, he contended, though substantially replaced during the last glaciation, might have mixed with the "Galley Hill type" enough to have contributed some genes to the formation of the "Nordic" type of modern European; similarly, the heavy-browed Rhodesian man (truly like a force acting at a distance) might also have mixed with early *Homo sapiens* groups and thus contributed to the formation of the "Australoid" population.[92]

Similar loose reasoning and inconsistency afflicted Hooton's handling of other issues involved in the vexed question of the Neanderthals. In discussing the determinants of ape and hominid skull form he implied that there was a functional explanation for the large brow ridges in Neanderthaloid skulls, by noting that "snouty" forms of mankind had employed these ridges as bony supports for their highly developed jaw muscles. At the same time, though, he felt compelled to reject the idea that these muscles and the ridges that accompanied them were necessary for dietary or other adaptational reasons. The intention here was apparently to preserve Piltdown man's combination of smooth brow and large jaw as a functionally viable one.[93] Yet the Piltdown skull was not truly compatible with this formulation of the issue, either, for if one made the reasonable assumption that large jaws must have had large muscles attached to them, then on Hooton's own hypothesis large brow ridges would have been expected for mechanical reasons, questions of diet and adaptation aside.

Another example of Hooton's straining of his evidence in regard to the Neanderthal question came in his discussion of the replacement of the European Neanderthals by their Upper Paleolithic successors. In trying to explain the great variability of the skeletal material uncovered at the Yugoslavian site of Krapina,[94] he attributed it to a mixture of Neanderthal bones with those of "modern human types," an identification which indicated to him "in no uncertain manner that the Neanderthaloids in this region were eaten by their more highly evolved successors."[95] The facts, stressed by Hrdlička in his discussions of Krapina, that all the bones present, and not those of "classic" Neanderthal morphology only, showed similar evidence of breakage, and that only Mousterian artifacts had been found with the fossil material, did not deter Hooton from this comforting speculation.[96]

As far fetched as some of these ideas about human evolution might seem today, it is important to note that Hooton's views would have to be classed as moderate within the 1920 - 1935 context. Much like MacCurdy's, Hooton's discussion of human fossils was meant to navigate between extreme positions -- to avoid being too strong a partisan or denigrator of the importance of any of the major fossils then accepted as authentic hominid forms, as well as to construct a human "family tree" that gave all the fossils a meaningful place. The dominant assumption behind the overall interpretation -- that Homo sapiens had probably made its first, though so far undocumented appearance, early in the Pleistocene -- was also a "moderate" one that had been explicitly rejected in America only by the maverick Hrdlička. The other thematic keynote of Hooton's treatment of human evolution -- the belief that the brain had led the way in human emergence, and had reached a "supra-ape" level of development before the perfection of other human adaptations -- had become a commonplace idea since Elliot Smith's championing of it in the pre-World War I era, though perhaps few writers stressed it so strongly as Hooton did.[97]

Just as interesting as the discussion of fossil hominids in Up From the Ape was the way in which Hooton tried to incorporate material from ontogenetic studies into his analysis of human evolution. The best evidence he had on this subject came from then recent studies conducted by Adolph Schultz (1891 - 1976) at Johns Hopkins on comparative primate embryology, for Schultz had showed a way out of the sterile search for atavisms and other evidences of recapitulation that had been a conditioned reflex in much anthropological writing on ontogenetic problems.[98] Schultz, following along lines laid down by Darwin and von Baer,[99] had stressed the idea that embryological data could shed light on the path that evolution had taken beyond that revealed by the "transient repetitions of ancestral features" so dear to the recapitulation theory. There was another entire class of comparative data, which, in Hooton's summary of Schultz' findings, was said to reveal "such close similarities in the details of the process and structures of embryonic life that they prove evolutionary relationship, without, however, contributing in any way to a hypothetical reconstruction of ancestors."[100] Hooton drew on Schultz for specific illustrations of the second category of evidence, so that in addition to the hoary repetition of ways in which humans showed "transient repetitions" of their quadrupedal ancestors,[101] he could provide interesting examples of what he called embryonic "parallelisms" between humans and various hominoids in characters such as the shape of the thorax, proportions of the limbs, and the size of the head and brain relative to other parts of the body.[102] The last example was probably the most important because it opened the whole issue of differences in growth timing as a key to the evolutionary transformation of apes into humans.[103]

If humans shared what had previously been thought of as some of their distinctive features with the embryonic and newborn stages of other primates, it followed that tracing the later stages of primate ontogeny might give further clues of human evolutionary relationships. On this issue, however, Hooton had less good data to draw on; he noted that as he was writing his book there had been as yet no records made of the postnatal growth of great apes of known age that could be compared with corresponding figures for humans. Still, he could, and did, cite some indirect measurements made by Keith which revealed interesting possibilities. First, the fact that the great apes appeared to share with humans a long gestation period and a great "prolongation of infancy" as compared to other primates seemed to Hooton to buttress the hypothesis that there had been a relatively recent common ancestry between them and humans.[104]

The belief in a close evolutionary relationship was further confirmed by Keith's suggestion that differences in growth timing between the facial area and the skull vault were a major determinant of the different skull forms assumed by adult apes and humans. The gorilla, with a relatively large brain case at birth, was said to grow steadily and slowly in this feature, while humans retained a very rapid rate of growth in the brain case from birth until the fourth year of life, and only then began to slow down. In addition, Hooton noted, the human face apparently started to grow most rapidly after the period of maximum brain growth, while in great apes facial growth commenced earlier, and proceeded more rapidly and for a relatively greater segment of the maturation process.[105] Hooton also claimed that the studies of Robert Yerkes of great ape psychology fit in nicely with the evidence on cranial growth processes -- chimpanzees for example were said to be "more precocious in their maturation" than humans, but while "differently timed" their psychological growth pattern was apparently "roughly analogous" to the human pattern.[106]

For several important writers of the period -- in Europe Keith and Ludwig Bolk (1866-1930), and in America Gregory and to an extent, Hrdlička, data like this provided convincing evidence that the phenomenon of "neoteny" or "fetalization" could account for many differences between humans and their great ape relatives, i.e. that many important human characters had resulted from the prolongation of fetal growth rates characteristic of higher primates generally into later stages of human ontogeny. Significantly, Hooton himself would not go so far as to endorse the theory of neoteny fully; for him it sufficed that the ontogenetic data fit in with his observations about comparative anatomy, by appearing to show that humans were specialized for brain growth, and apes for expansion of the masticatory apparatus.[107] This emphasis on divergent specializations did not deter him from endorsing Keith's theory that endocrine differences were "probably" the single most likely mechanism involved in producing these ape-human distinctions.[108]

Another example of Hooton's groping toward new ways of understanding human evolution appeared in the discussion of primate reproduction in Up From the Ape. In this area as in others, Hooton wanted to stress basic similarities in pattern between the great apes and humankind. Thus, he asserted, the fact that both groups exhibited small litter sizes and large neonatal brain sizes was no coincidence, nor were other similarities like delayed sexual maturation and intensive parental care of the young. All were interrelated, he argued, as ways in which both humans and apes could insure the high intelligence in the individual adult that guaranteed survival. In a crude analogy similar to the recent theory of "K-selection,"[109] Hooton speculated that "since all the eggs are put in one basket, or, more accurately, since the basket will hold but one egg at a time, that basket must be watched very carefully and its contents assiduously cherished."[110] The ape-human similarity in such "cherishing" behavior, Hooton noted, even went as far as male protection of, and provision of food to, the young, which he claimed had been observed in gorillas and chimpanzees. Still, he reminded his readers, these similarities must not blind one to the fact that "the full dignity and responsibility of paternity was attained only when man became an erect-walking and ground-dwelling animal."[111] The large quantitative gap that he had been noticing all along thus had to remain in this area as well.

In one important aspect of Up From the Ape Hooton did appear to be narrowing a biological gulf that previous generations of physical anthropologists had taken for granted, i.e. the supposed gulf between whites and other human races in general mental capacity. Partly, this caution must have reflected that fact that in the Boasian era a professional anthropologist could not afford to parrot the conclusions of the eugenics movement uncritically, but it was also a product of Hooton's own scepticism about received wisdom, and his belief that most racial differences were in non-adaptive characters.

One must stress, however, that this scepticism about racial psychological differences was a partial scepticism -- Hooton was quite willing to accept the existence of temperamental differences among races, even the most stereotypical ones, and even to accept differences in levels of specific intellectual abilities. For example, at one point he asserted that the "Armenoid" type within the white race (to which many Jews allegedly belonged) was "associated with a positive genius for commerce and an infinite capacity for material and intellectual advance under the most exiguous environmental opportunities."[112] Yet, even though the discussion of the major racial groups in <u>Up From the Ape</u> contained several examples of the acceptance of cultural stereotypes as biological facts, Hooton did not make invidious comparisons concerning overall intelligence and adaptive abilities among the major divisions of humanity -- the White, Negro and Mongoloid races. By implication he was opting for the conclusion of "separate but equal,"[113] and he did make a point of explicitly rejecting the notion of "Nordic" supremacy that had achieved such wide currency during the 1920s.[114]

The most interesting remarks in Hooton's discourse on race were those about the Negro. He refused to place it in the group of "less advanced" races as he had done to the aboriginal Australians and the central African pygmies, though his characterization of the Negro was not free of familiar stereotypes -- the general conclusion was that Negroes represented "a highly specialized and fully human type, patient in adversity, exuberant in prosperity, with certain special gifts and talents, some weaknesses, no doubt, but possessing a superior capacity for biological survival."[115] Hooton also pointed out several arguments that undermined the invidious comparisons that had often been made to the detriment of Negroes in the United States. First, he noted, despite their lowly status in America Negroes had had major cultural and political achievements in Africa, and races with a probably significant proportion of Negro blood (among which Hooton counted the ancient Egyptians) had also achieved much. The African Negroes' cultural sophistication seemed all the more impressive to him when he considered that the African tropical forest, the center of the Negro population zone, had not only been cut off from the main culture areas of Eurasia, but was itself "peculiarly unfavorable" to cultural development.[116]

Finally, Hooton questioned the major piece of hard data that had been compiled against Negro mental abilities, i.e. their poor performance on standardized intelligence tests, since he believed the latter to be "ineradicably permeated with the environmental flavor of European civilization."[117]

While Hooton questioned the traditional wisdom of European and American racism, he was not willing to associate himself with the "psychic unity" theory that he saw as the reigning orthodoxy among cultural anthropologists.[118] When better tests had been designed and racial boundaries fixed more accurately, he was certain that real psychological differences between blacks and whites would be found, and he left open the question of whether these differences would be capable of arrangement on a scale of higher to lower ability. Also even if the Negro, so far at least, was not, there still were in his view some stepchildren in the extant human family. As the analogies he had made in discussing ape-human differences would lead one to expect, these unfortunates were the Negritos, defined to include the Andamanese people of southeast Asia as well as the pygmies of central Africa, and the Australian aborigines. The Negritos Hooton characterized as the "backward children of mankind," pushed into isolated corners of the world by conflict with superior races, while the Australians he portrayed as "contemporary ancestors" possessing a "fossilized society" and "exiguous brains."[119]

Hooton's conviction on this point was strong, even though the objections he had made against conventional claims about Negro inferiority could have applied to his position here as well. For example, the pygmies could have been counted as "highly specialized" and thus well adapted to survival in the difficult environment of the tropical rain forest; also, the relative backwardness of Negritos and Australians could have been explained by relative isolation from the main areas of civilization even more easily than that of the Negro. Finally, his strictures about the dangers of applying culturally biased criteria of intelligence seemed to be even more to the point in these cases than in that of contemporary Afro-Americans.

That Hooton would not apply these strictures to Negritos and Australians reflected the limits of his ability to achieve a sympathetic understanding of "primitive" peoples; he apparently had a difficult time seeing that the way of life of small bands of hunter gatherers, for example, demanded much in the way of mental ingenuity or cultural sophistication. This blind spot continued to influence Hooton's scenarios of human evolution in the writings that came after Up From the Ape, as well as his conception of the kinds of evidence that could be used to reconstruct the evolutionary process.

Making Adjustments and Treading Water -- the 1930s

In the discussion of race as in other sections of Up From the Ape, Hooton revealed himself as a creative, and often critical, synthesizer of the various theories and lines of evidence that surrounded the study of physical anthropology in the 1920s. In his writing about human evolution after 1931 he attempted to continue in that role, but the results he achieved marked him ever more clearly as a transitional figure whose basic perspective was increaingly at odds with the direction in which the study of plaeoanthropolgy was moving. It seemed almost as if by the early 1930s critical analysis of received ideas had substantially ended; having sifted the latter, he had made his selection of those with which he could live comfortably. While he continued to keep abreast of new evidence, methodological concerns, and phylogenetic theories, he kept these "comfortable" ideas at the center of his thought. He tried to harmonize the old and the new whenever possible, but sometimes the price of superficial harmony was a deeper incoherence. Hooton obviously took his duty to inform people about new developments very seriously, but his own conclusions about them came to appear more and more strained and antiquated.

The decade of the 1930s provided plenty of grist for Hooton's mill, for it was a time of major expansion in the store of hominid fossils, especially in regions outside Europe that were just beginning to receive intensive study by paleoanthropologists. Whenever Hooton chose to discuss new discoveries, however, it was within the guidelines laid down in his earlier work -- in particular the themes of multilinearity and the early appearance of Homo sapiens. The mid-1930s alone saw several essays that touched on these subjects, essays which were given greater prominence by being collected into a major section of a popular book Hooton published in 1937 -- a book felicitously, if stagily entitled Apes, Men and Morons.[120]

In some of these essays, such as the one first published under the title "Homo Sapiens -- Whence and Whither" in 1935, Hooton largely retraced the evidence and arguments familiar from his earlier work. For example, he continued his criticism of Elliot Smith's arboreal theory, especially as amplified by Wood Jones, and labelled it as "a sort of Just So Story of primate evolution," rife with

"Lamarackian lucubrations."[121] He also stated clearly his preference for Gregory's theory of a common origin for both the human and African great ape lines in a "generalized Miocene ape," as well as his preference for taxonomic principles different from those of Gregory -- with Hooton again placing his faith in the tracing of resemblances in "non-adaptive hereditary features."[122] Hooton made plain as well his continuing belief in Keith's principle that "human evolution has been a multiple and asymmetrical process," which had produced several distinct genera and species of humanity, and among which neither "Pithecanthropus," Heidelberg Man, nor the Neanderthaloids were to be counted as direct human ancestors.[123]

Elsewhere in Apes, Men, and Morons Hooton went beyond a restatement of the issues and expanded on the reasons behind his acceptance of the early Pleistocene Homo sapiens theory. First, he argued, anthropologists often underestimated the time necessary for the evolution of human biological characteristics because of a mistaken analogy with the swift pace of cultural evolution over the last several thousand years. Even geologists and zoologists, he contended, had not shown a proper appreciation of the slow pace of biological evolution until recently. To illustrate this idea he pointed to the then novel development of a "radioactive clock" based on the decay of an isotope of the element thorium; this clock he noted, had produced probable durations for the Pleistocene and Pliocene of one and six million years respectively, durations far longer than previous estimates based on measures like rates of sedimentation. Interestingly, this point could also have worked against the early sapiens theory as well, since it would have extended the period of time available during the Pleistocene for the final steps in the emergence of anatomically modern forms of humanity, but Hooton ignored this problem. Hooton, like MacCurdy,[124] also argued that the then extant cultural evidence, especially the relatively late occurrence of typologically well defined tool traditions, did not count against his theory. No, he asserted, there had to have been "a very long period of tool using by early men or proto-men before the stage of typologically well differentiated stone industries was attained. Eoliths must have been used far back in the Pliocene." For this reason he could accept the findings of archeologists who were then suggesting that the "Chelles-Acheul" tool tradition had not appeared until after the Second Glacial epoch, and still hold out for the early Pleistocene appearance of Homo sapiens.[125]

In a 1939 collection of essays devoted mainly to contemporary biological and social problems, Twilight of

<u>Man,</u> Hooton also continued to promote the view that human behavioral evolution had been in great measure a self-initiated process. A bit defensively, he took note of the fact that others saw it as a product of chance factors like hereditary variation and environmental pressures, and even admitted that these factors had probably played some role.[126] Still, he persisted in portraying the transition to ground dwelling among early hominids as largely "a matter of choice and willingness to risk safety in order to secure a fuller existence;" humans' upright posture, he said, had been "achieved only by the persistent efforts of animals whose nascent intelligence had made them realize, however dimly, that an upright stance and bipedal form of locomotion would enable them to get their noses off the ground, enlarge their horizons, and set free their upper limbs for prehension and for the use of tools."[127] He could thus conclude that man, "aided perhaps by natural selection and a few environmental breaks," had "lifted himself by his bootstraps from the status of ape to humanity."[128]

Mere restatement, of course, could not alone suffice in establishing Hooton's views of human emergence; there were still problems to be surmounted, and Hooton recognized some of them. The major one, that the only fossils then commonly referred to the early Pleistocene, Piltdown man and "Pithecanthropus," were in differing ways much more primitive than any early sapiens would have been, was familiar, and a familiar answer sufficed -- to assign a "typical," and earlier date for these fossils based on their morphological status. Thus, Java man, as "the most archaic humanoid type" should, he argued, be taken to "represent the survival of an early Pliocene or late Miocene type;" Piltdown, a "woman" with "a virtually full-blown human brain," still had to be classed, because of her primitive jaw, "almost certainly as the superannuated survival of a Pliocene type."[129] That Hooton would perpetuate this time honored custom was perhaps not surprising; what was more interesting, though, was that he did not employ it consistently. For example, when writing in the same volume on the issue of human antiquity on the continent of North America, he warned of the pitfalls of the practice of morphological dating when dealing with specimens like the so-called "Minnesota woman." In such cases, he asserted, "unreasonable morphological restrictions" should not be imposed in trying to fix a specimen's date; rather, he said, "the acid test of their antiquity must be geological."[130]

The early sapiens theory, Hooton recognized, also faced challenges from some of the most important fossil evidence that had accumulated between 1925 and 1935.

Peking man, for example, could function perfectly as a transition form between "Pithecanthropus" and the Neanderthaloids, and by 1937 was being interpreted by Franz Weidenreich as having several key characters in common with the Mongoloid branch of _Homo sapiens._ [131] Hooton, however, at first rejected the validity of the latter suggestion, and asserted that the former point did his theory no real harm. After all, he had always argued that the Neanderthaloid group had been evolving in parallel with contemporary populations of _Homo sapiens._ [132] More serious, he thought, was the support that Rhodesian man gave to the theory of a "generalized Neanderthaloid" stage in human evolution, especially after the discovery in 1931 of an apparently similar type, the so-called "Homo soloensis." To Hooton, though, these finds only indicated the development during the Pleistocene of "a number of coarse-boned types of men with big brow ridges and low foreheads but otherwise not particularly closely related" to each other, or to _Homo sapiens._" [133]

On the other morphological end of the "Neanderthaloid stage" issue were the fossils found in the Skhul cave at Mt. Carmel. McCown and Keith, Hooton noted, were already beginning in 1937 to advance the theory (though the full monograph in which the theory was developed, and modified, appeared in 1939) that this population represented a stage in the process of transformation from a Neanderthaloid form toward Homo sapiens. Again, however, Hooton explained away the new evidence, asserting that the Skhul fossils probably represented a hybrid between a pre-existing Homo sapiens population and the more typically Neanderthaloid population represented by the skeleton found in the Tabun cave, also at Mt. Carmel. Despite his primitive appearance, Hooton gibed, Neanderthal man "may have been a good mixer."[134]

Hooton apparently felt that he had been able to account for all the difficulties in the recent fossil evidence, but there was one in the cultural evidence that he failed even to consider -- the so-called "chopper culture" of Peking man. In the mid 1930s reports were coming out of China documenting the existence of a "typologically well defined" tool tradition among these supposedly primitive survivors of Pliocene morphology.[135] Why, one could ask, should the so far undiscovered anatomically modern Homo sapiens population of the early Pleistocene have been using tools more primitive -- i.e. "eoliths" -- than those used by the smaller brained "Sinanthropus" of the middle Pleistocene? The implication clearly would have been that inferior brains could produce superior cultures, which would not have been a possibility that Hooton would have found comforting.

Clearly, the only way to have dealt adequately with the difficulties in the early *Homo sapiens* theory would have been to produce anatomically modern specimens of undoubted geological antiquity. It was the absence of these, in tandem with the continuing finds of "coarse-boned" types that was pushing paleoanthropologists, even Keith himself, into what Hooton called a "premature" retreat from their "paleontological Verdun."[136] Hooton's hopes for fossil "reinforcement" of his position had in fact been raised in 1934 by an announcement from an unexpected quarter -- the young Louis Leakey's discovery of allegedly early *sapiens* fossils in Kenya. When Leakey fully described his Kanam and Kanjera specimens in 1935, Hooton pronounced himself basically convinced that Leakey had found evidence of *Homo sapiens'* presence at least as far back as the Middle Pleistocene. Still, Hooton noted several qualifications that made him less enthusiastic than he obviously wanted to be -- namely, Leakey's "ill-considered" redefinition of the stages of the African Pleistocene, his vagueness in describing the geological context of his discoveries, and finally, Leakey's insufficient knowledge of physical anthropology, which seemed to Hooton to be at the "gifted amateur" rather than the professional level.[137] By 1937 Hooton was having to qualify his support still further, recognizing that Leakey had been premature in some key assertions he had made about the modern form of the Kanam mandible. Hooton still continued to believe, however, that "Dr. Leakey was probably right and that he actually did find the ancestor of man in an early Pleistocene deposit."[138]

By 1940, when he published his next volume of essays,[139] Hooton felt that he had more reliable evidence at hand, evidence that was sure to dispel all the lingering doubts that surrounded the early _sapiens_ theory. His centerpiece was another British fossil, the skull found at Swanscombe in 1935. Here, finally, was a discovery backed up by meticulous geological, paleontological and morphological analysis that had been performed by a committee of specialists -- an analysis which revealed what Hooton characterized as a "modern" type of skull without Neanderthaloid or "pithecanthropine" affinities, found in undoubted association with Middle Acheulian hand-axes in a deposit of the Second Interglacial epoch.[140] In addition to this critical fossil evidence, there was apparent support for the theory of multilinear hominid eveolution from what he claimed were recent refinements in the understanding of Paleolithic tool types and their distribution in space and time. Specifically, he could point to the "chopper culture" of "Sinanthropus," which he now clearly recognized as of middle Pleistocene age, and claim that the absence of "true bifaces" from this culture complex indicated that this crude culture had developed separately from and in parallel with the Acheulian. Only, he noted, in the "Soan" culture of India, which had been probably a zone of transition or contact between the races that had produced the divergent tool traditions, were choppers and handaxes present in the same assemblages.[141]

There were also other pieces of recent archeological evidence which Hooton took to be support for the existence of separate lines of hominid evolution in the Pleistocene. First was the contention that the earlier Neanderthaloids of Germany had possessed a "proto-Mousterian" culture similar in several ways to the "Sinanthropus" culture. Another was the supposed fact that no Neanderthaloid fossils had ever been found in an Acheulian cultural context. Finally, following Henri Breuil, he noted that so-called "Levallois" flake tools, which had so often in the past been lumped with the Mousterian flakes made by Neanderthal man, were actually produced by the same peoples who had made later Acheulian hand-axes; their presence at sites in Europe thus indicated that physical types other than the Neanderthaloids had also been present. In this context, the finding that both Mousterian and Levalloisian flake tools had been present in the cases of Mt. Carmel gave additional credence to Hooton's hypothesis that the Mt. Carmel population represented an instance of hybridization between Neanderthaloids and Homo sapiens.[142]

The practice of identifying different tool traditions with morphologically distinct lines of hominids was a common one among prehistoric archeologists,[143] but Hooton had not used it extensively in his previous work. Apparently, he felt that he had to justify the practice, and interestingly, he did so in a way that showed a new concern with Darwinian mechanisms of evolution -- mechanisms which he had found so woefully insufficient to explain human emergence at other times. "I am not so credulous," he asserted,

> as to conceive of any direct causal relationship between anatomical minutiae and the idiosyncrasies of human technology. The idea is, rather, that in the early types of man, human behavior, including technology, evolves with the organism. The organism is modified through isolation, selection, in-breeding, and spontaneous variations, and so is the behavior of the animal including his technology. Both are adapted to his physical environment.[144]

Despite the concreteness of this way of putting his thesis -- it was definitely far less vague than the concept of "initiative" -- a Darwinian account of parallel hominid phyla raised more problems than it solved. A similar problem, as we have seen,[145] plagued Osborn's attempt to provide an adaptive basis for multiple lines of hominid descent in his own writings. For instance, in what ways were the two major cultural traditions adapted to their particular physical environments, and especially, what differences in adaptation were to be inferred from the differences in tool form? Also, though it might sound plausible to discount causal relationships between "anatomical minutiae" and the cultures that evolved in tandem with them, there was one "minutia" -- the supposedly more advanced brain of the _sapiens_ line -- that ought to have had some particular cultural outcome. Perhaps aesthetic fascination with the symmetrical hand-axe versus the "crude" chopper would have been enough of an outcome for the Hooton of 1925, but the Darwinian Hooton ought to have been able to show some adaptive superiority. And if adaptive superiority had existed, why had the "backward" population not been replaced before the height of the last glaciation?

Hooton's implication that the two lines of culture and the races supposedly associated with each had developed in isolation from each other also was problematical. Perhaps the separation between the

Acheulian and the Asian "chopper" culture held up, but could the later Acheulian be so clearly separated from the Mousterian? If mixture between the tool traditions did occur, must some hypothetical racial hybridization be held to account for it, or did it make more sense to argue that among hominids, creatures marked by learning abilities superior to other animals, tool making behavior was more likely to have spread through cultural rather than genetic diffusion? Finally, a strictly empirical question, posed by Hrdlička over a decade earlier, still remained -- what had happened to the descendants of the "hand-axe people" during the long period when the Mousterian was the dominant culture, and the Neanderthaloids were the dominant racial group, in the Old World?[146]

Hooton probably could live with some weaknesses in his Darwinian explanation, for the new morphological evidence provided by the Swancombe skull seemed to be strong proof of the fact of multilinearity, however one might explain that fact. There were two difficulties with the Swanscombe material, however, one that Hooton noted obliquely, and one that he failed to note at all. First, to say that Swanscombe man and Peking man were both Middle Pleistocene forms was not the same as saying that they had been contemporaries. As Hooton pointed out in another context,[147] the longer estimates for the duration of the Pleistocene that were appearing indicated that the Second Interglacial might have lasted as long as 200,000 years. This was clearly enough time for Homo erectus to have evolved into an archaic form of Homo sapiens, unless one believed the change already to have occurred. More important, though, Swanscombe was not a complete enough specimen to bear the full weight of the early sapiens theory. With the frontal portion of its brain case and its face missing, it could not be assumed to have possessed anatomically modern proportions in these regions a priori. Also, while Swanscombe's occipital region did seem more like modern forms of humanity than either Homo erectus or the "classic" Neanderthals, contemporaries of Hooton had already noted that it did not differ greatly from the early Neanderthaloid Steinheim skull, either.[148]

The apparent dogmatism that Hooton showed in adhering to his original views about fossil humans and in trying to bend new evidence to those views seems a bit paradoxical when contrasted with his ideas about modern races, which continued to show scepticism about traditional notions regarding the interaction of race and culture. True, Hooton did stick to the idea that definable racial types existed, and that races possessed typical behavioral characters as well as morphological ones. Also, he still argued that in relatively isolated, "pure" types like Bushmen, Australians, and Eskimos both classes of characters could be identified. Nevertheless, he departed from traditional racist views by asserting that, while many of these characters might be identified as less "advanced", in the sense of "distant from ancestral conditions," than those found in groups from less isolated regions, it was "by no means clear that any causal relationship" obtained among these races "between the inferiority of their material and non-material culture and certain archaic biological patterns they preserve."[149] He was also willing to recognize that among the main divisions of humankind cultural diffusion, racial mixture and variations within each race in the level of civilization attained had made the identification of correlations between race and the capacity for cultural achievement an unrealistic goal. This now seemed to Hooton to be especially true if the goal of the search for correlations was to rank races on some sort of ascending scale of ability.[150]

Similar conclusions seemed to emerge when Hooton chose to examine other types of evidence relating to the interaction of race and culture. In looking at the history of Homo sapiens in Europe from the Upper Paleolithic to the early Iron Age, for example, he could see no physical changes of probable genetic nature that could have been correlated with the rapid evolution of culture. In comparing modern "civilized" urban populations with those of the same racial background living in rural or "uncivilized" conditions, he could similarly identify no major psychological or physical differences of probable genetic origin. The functional differences that he noted seemed to him to be ontogenetic in origin; the one exception was that certain hereditary malformations like those of the teeth were more prevalent perhaps among civilized groups, since such defects were less likely to interfere with survival under civilized conditions of life.[151]

That Hooton was so hard-headed in his devotion to parallel phyla and early Pleistocene Homo sapiens seems even more out of place when one views it against his general appreciation of the need for caution in advancing theories of human phylogeny -- an appreciation that reflected his understanding of the many sources of bias that could affect the thinking of physical anthropologists, and his awareness that new types of evidence had to be gathered before the study of human evolution could become a mature discipline. That these concerns were of importance to him is indicated by the fact that he chose them as subject matter for his own contribution to the major Conference on Early Man held in Philadelphia in 1937, a gathering that attracted an international cast of luminaries in the field of paleoanthropology. This essay, "Biology and Fossil Man," also deserves special attention for the interesting way in which it combined perceptive analysis of factors that impeded progress in the understanding of human evolution, sound predictions about the issues that would have to be studied in the years to come, and some special pleading for Hooton's own rapidly aging theories.

A large part of the argument in Hooton's paper was devoted to the limitations that the biases of investigators had placed upon free debate and rational analysis of human evolution. In introducing the subject of bias, Hooton could not refrain from a slap at his Boasian colleagues in cultural anthropology, who, he claimed, paid too much attention to living "primitive" peoples and not enough to early man. After all, he contended,[152]

> the achievements of fossil man entitle him to more consideration, certainly, than modern savages deserve by virtue of the lack of achievement which commends them to anthropological attention. Fossil man invented the first tools and discovered the use of fire; he was probably the originator of articulate speech. He made himself from an ape and created human culture. If his successors have accomplished anything more substantial, I am not aware of it.

ANCESTORS OR ABERRANTS
STUDIES IN THE HISTORY OF AMERICAN PALEOANTHROPOLOGY, 1915-1940

A Dissertation Presented

By

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The first strictly paleoanthropological bias that he identified, however, was the common habit of allowing early discoveries to shape preconceptions about what later discoveries ought to be; by imposing such "blinders" on oneself, he noted, one ran the risk of rejecting crucial new evidence when it appeared, because it did not conform to expectations. His example was the aftermath of what he chose to call the "premature discovery" of Neanderthal man; once these fossils had been received as a perfect intermediate stage between presumably ape-like early hominids and Homo sapiens, Hooton argued, all "heretical and non-conforming fossil men" had been unceremoniously "banished to the limbo of dark museum cupboards."[153]

The way Hooton developed this point -- a valid one in the abstract -- was obviously meant to justify his acceptance of modern-looking "heretical" fossils like Galley Hill man. What he had neglected to point out, though, was just as obvious -- i.e. the fact that most of the "banished" heretics to which he referred had geological marks against them as well as morphological ones. The morphologically most dubious of them all, Piltdown man, had largely been accepted, at least in England and the U.S., because of the widespread belief that it truly was of great antiquity, and because it conformed to other theoretical expectations than those built upon the Neanderthal fossils, expectations that were not based on fossil evidence, however.

The second source of bias that Hooton identified was the existence of national traditions in the study of fossil humans. The Germans, he asserted, were "pro-ape," in the sense that they preferred the theory of close evolutionary relationship between humans and the existing great apes above other evolutionary scenarios. In his view this attitude inclined them to dissociate all fossil fragments with simian characters from those with human ones, even if they were found in the same deposit. The implied link in this somewhat elliptical chain of reasoning was the example of the Piltdown fossils -- Hooton apparently felt that the "pro-ape" sympathies of the Germans caused them to expect early hominids to be uniformly ape-like, and thus to resist Piltdown's disharmonious combination of ape-like and modern human features.[154] The French, though allegedly less dogmatic about evolutionary scenarios, he portrayed as "constricted and noncommittal" in their evaluations of putative human ancestors. They also, he believed, held too strongly to the practice of interpreting fossils as prehistoric supports for contemporary national pride -- the most conspicuous example having been the French myth of a homogeneous Cro-Magnon race. Still, Hooton felt obliged to point out the excellence of some individual contributions by French scientists, and showed his agreement with other American paleoanthropologists in singling out Boule's monograph on the La Chapelle-aux-Saints skeleton as "probably the most nearly perfect study of a geologically ancient human specimen" yet produced.[155]

Anglo-American science came in for a share of Hooton's criticism as well as continental European efforts, but a lesser share. Americans, he said, had apparently agreed on an anthropological "Monroe Doctrine" in refusing to consider the Old World theory of cultural diffusionism. In physical anthropology he noted as a national trait the scepticism, largely justified in his view, about the presence of Pleistocence human populations in North America, a scepticism which he gave Hrdlička the principal credit for promoting. Still, he chould not help questioning whether this scepticism had not gone too far sometimes -- the case of the so-called "Minnesota woman" was the example he gave of the pitfalls of overzealousness in defending a fundamentally sound idea.[156]

Regarding the English Hooton had the following observations. Unlike the Germans, British scientists generally favored an early separation of hominids from the anthropoid stock. They also had a predilection for functional and mechanistic theories of primate evolution (once again Elliot Smith and Wood Jones were singled out), theories that Hooton said had been "employed at times with less caution than enthusiasm." He also detected a "sporting attitude" about the British that they displayed in their ready acceptance of new discoveries, an admirable trait that sometimes, though, went so far as to become rashness.[157]

Where, one might ask, did Hooton himself fit in this typology of national styles? He did not characterize himself, and his status as an American did not leave much to go on, since his remarks about America concerned specific issues off the main line of paleoanthropology. It seems pretty clear, though, that apart from his squeamishness about functional theories, Hooton fit in very much with the English and their "sporting attitude." Perhaps his omission of an overall American style made sense -- not only were the disparities among the best known writers significant, but also most of the work done by Americans had been filtered through a wide range of European influences. The perspective that each writer adopted was thus more a complex, and highly personal, amalgam than a national viewpoint.

Indeed, it was to more personal sources of bias that Hooton turned next, though he categorized these rather narrowly as "individual psychology" when a more inclusive term like "professional ideology" might have expressed his meaning better. At any rate he managed to point quite accurately to several intellectual tendencies that often marred accurate evaluation of fossil hominids (including his own evaluations, alas). The first, and perhaps most significant of these tendencies was in Hooton's view the habit of "aggrandizement of a rare and unique specimen" by overemphasizing its supposedly "peculiar" features and neglecting to notice the range of variability in the same characters of related fossil forms. Most often, he opined, this habit took on two opposing variants -- on the one hand, it emerged in an overconcentration on supposed "simian" characters of fossils that made possible hair-splitting taxonomies; on the other, in the use of truly insignificant morphological features as evidence of close genetic relationships between the fossil under analysis and later forms of humanity.[158]

The second objectionable practice that Hooton identified was a form of professional hubris in which anthropologists oversimplified the paleontological and archeological problems associated with the interpretation of particular fossils, while paleontologists and archeologists offered facile accounts of the anthropological issues they encountered. To him, the best corrective for these problems seemed to be team effort, where experts from each relevant field analyzed the appropriate aspect of a discovery, and left other aspects to more qualified collaborators.[159] A then recent example of this procedure which he was able to cite was the "Sinanthropus" expeditions at Choukoutien. But even specialization and expertise were no guarantee of accuracy, Hooton reminded his listeners, when a scientist was working under the narcotic influence of pet theories. "Perennial consistency in the views of an anthropologist," he warned, "is synonymous with stubborn persistence in the wrong. If you do not change your mind it petrifies."[160]

Along with delivering these Emersonian injunctions, Hooton counseled his colleagues against other bad habits and biological fallacies that prejudiced the accurate interpretation of the incomplete evidence on fossil humans found so far. One habit, already alluded to above, was lack of appreciation of the range of human variability, a variability that could safely be attributed to fossil as well as modern forms of humanity. Another supposed error, one that he apparently never tired of mentioning, was the belief that all parts of the organism must evolve harmoniously, and at the same rate; this attitude, he contended, led to "rash reconstructions" and the "dismemberment of unfortunate fossil individuals," with the different parts being attributed to separate species or even genera. Of course it was Piltdown that the last phrase referred to; as always, what he neglected to mention was the need for some minimum level of morphological "harmony," the level required to show that a supposed "individual" could acutally have been a functioning whole adapted to a particular ecological niche.

His omission of such considerations, though, seemed to follow from another criticism he often made, and made again in this address -- that is, that anthropologists too often tried to intuit function from "anatomical minutiae" without sufficient experimental justification. As an example, he cited Tilney's conclusion (one which he himself had echoed in Up From the Ape [161]) that "Pithecanthropus" probably had been a speaking creature because it possessed a third inferior frontal convolution even though Broca, the discoverer of this "motor speech area," had found the convolution present in a chimpanzee which, Hooton noted ironically, "presumably could not speak."[162]

Finally, much as Gregory had often done,[163] Hooton criticized those who adhered dogmatically to supposed "laws" governing the evolutionary process. All too often, he believed, reliance on these principles had straightjacketed scientific imaginations, provided cover for personal biases, and failed to live up to the test of biological fact. The four laws he singled out were orthogenesis, convergence, parallelism, and saltatory mutation, with the first and last, in his view, being the major offenders against the facts. It was well that Hooton saw the invoking of parallelism as a less egregious habit than some others, since it was one that he implicitly relied on in his conception of several hominid lines, all developing larger brains and more complex cultures while remaining genetically distinct far back into the Pliocene.[164]

After delivering all these criticisms, Hooton tried to make constructive suggestions to his colleagues, and ended his address by outlining the areas of research potentially most fruitful for improving understanding of "late prehuman and early human biology." Especially promising to him seemed further research on the diet, posture and locomotion, social habits, and intelligence of living non-human primates; while further laboratory study would be important, there was in his view an even more pressing need to study these creatures in their natural environments. Useful data of a comparative sort was also still to be gained, he thought, from the study of human populations -- not only through research on human biology but also through analysis of the modes of adaptation of "primitive" tribes, and especially their technology. Regarding fossil hominids, Hooton emphasized the potential value of studies analyzing larger samples of specimens, such as that provided by the Choukoutien population of Peking man, in order to illuminate issues like growth and development and the extent of sexual variation in body form. He also expressed the view that more extensive study of the habitation sites of fossil hominids might provide a basis for sound hypotheses about diet and other aspects of their way of life.[165]

Living Primates and Human Evolution -- Man's Poor Relations

Hooton showed that his espousal of new approaches to the study of human evolution was more than a matter of the moment or merely advice for others, by publishing a major book on the functional study of the primates in 1940. As always irreverently titled, Man's Poor Relations was a synthetic work; in it he attempted to gather together the disparate strands of knowledge that had been accumulating over the previous twenty years concerning non-human primates. In general Hooton satisfied himself with descriptions of what others had found, though here and there he did try to weave strands into a partial fabric revealing a more general pattern. Hooton would not have been himself if he had not engaged in some phylogenetic speculation, but he confined his excursions into this area largely to the introduction and conclusion of his book.

Right at the outset of Man's Poor Relations Hooton set the tone for the entire discussion by showing himself still to be in the camp of Keith and Gregory concerning the close biological connection between humans and their ape relatives. Aside from habitual bipedal locomotion, he asserted that the only important ways in which man differed from his close cousins were "bigger brains on the anatomical side, articulate speech and the use of the hands in creating and employing tools on the functional side."[166] And as he continued to believe as well that both the use of speech to communicate ideas and the use of tools were not dependent on any peculiarities of the human vocal tract or hands,[167] it is easy to see that he still felt that the brain had been the leading factor in human evolution.

No wonder then, that even though he waffled a bit on the exact phylogenetic relationship between humans and "brachiating" anthropoids, Hooton was willing to put forward the same evolutionary scenario that he had first laid out 15 years earlier, namely that "some primate strain, in one way or another, got off to an evolutionary start with a much more generous endowment of brains and intelligence than numerous allied lines which ultimately give rise to the present apes and monkeys." Following the appearance of this favored primate line, he contended, "the original prehuman endowment of brains and intelligence was enhanced in the course of evolution up to the emergence of the 'erect and featherless biped.'"[168] As in his earlier work, Hooton, despite his recent flirtation with Darwinian rhetoric regarding fossil hominids, gave no selective or ecological explanation for the appearance of this superior endowment or its further elaboration. The possibility of miraculous intervention remained open, but Hooton did not speak of "design" as he had in Up From the Ape.[169]

In keeping with Hooton's fundamental position that qualitiative differences between man and his close relatives were few, the lengthy discussion of primate intelligence in Man's Poor Relations tended to focus on categories of behavior -- problem solving, insight, etc. -- in which the various higher primates differed from each other in degree of development only. Significantly, the experimental results that had been achieved up to that time were mixed, from Hooton's point of view, and did not reveal a clear picture of ascending levels of general intelligence among the monkeys and apes. Among the great apes, data on the gorilla and orang was especially thin, resting largely on work done with very small samples by Yerkes during the 1920s. The evidence that existed tended to point to differences in the way in which these creatures used their brains, but not to major distinctions in levels of overall ability.[170]

Chimpanzees had proven to be more willing performers, and more available as well. Thus, Hooton could point to several experiments of the 1930s that had increased understanding of chimpanzee intelligence, experiments mainly tending to raise estimates of their behavioral capacities.[171] But this information did not lend itself to the drawing of phylogenetic conclusions, when set against the important findings of Heinrich Klüver (1897-1979) on tool using behavior in Cebus monkeys, creatures which, Klüver argued, displayed "insight" in the same way that Köhler's chimpanzees had been said to do.[172] There was one sort of theory for which Klüver's findings could have given some support -- the hypothesis that only man among the primates could be brought to use tools under anything other than "artificial" conditions. While it would have fit nicely with his belief in the distinctive qualities of "pre-humans" versus apes, Hooton did not raise the possibility, perhaps because it would have made ape-human differences too much a matter of kind rather than degree.

The other well known category of behavior in which humans might be deemed distinct in kind from their relations was of course speech, and here Hooton found even less basis on which solid conclusions could be raised. The best information available to Hooton was that provided by C.R. Carpenter (1905-1975) in his field studies of gibbons -- the conclusion of which was that vocalization did seem to play an important part in coordinating gibbon group activity, but seemed to be mostly emotional in motivation and resistant to alteration by learning.[173] In some captive gorillas Carpenter had also noted variations in the pitch and rhythm of vocal sounds that seemed to function in the same way as facial gestures, i.e. as expressive of the emotional state of the subject, but without ideational content.[174] A similar pattern of vocalization, Hooton noted, had earlier been observed in chimpanzees, and about orangs all he could say was that there were "apparently" similar sounds made in the wild.[175] Regarding the attempts that had been made up to that point to teach captive apes to use human speech, results also tended to be negative -- extremely limited results had been achieved, even after great effort.[176]

The bastion of articulate speech, which Hooton defined as the conveyance of "fact and idea from one individual to another"[177] still held out, and thus one clearly qualitiative distinction appeared to remain.

Unlike the study of primate intelligence, which by 1940 had had a generation of experimental work behind it, the analysis of primate social behavior had only truly emerged in the 1930s. Already, however, Hooton realized its great promise. In fact, he stated his belief that it might become as important in understanding humankind as the anthropological study of supposedly "primitive" societies, the characteristics of which, he asserted, were often not "basic" to humanity in general but unique to that type of community. Scientific enthusiasm had to be reserved largely for the future of the new discipline, however; while Hooton felt that the data on the "family life" of primates in captivity was reasonably good, the corpus of careful studies on primate societies was still meager, and several of these had employed captive groups as subjects.[178]

Given these limitations, Hooton still felt able to identify two subjects about which useful generalizations could be made -- territoriality, and sexuality and dominance. The latter issue had attained major importance in primate studies from the emphasis that the British primatologist Solly Zuckerman (b. 1904) had placed upon it in his work during the 1930s. Both aggressive sexuality and vicious displays of dominance had been prominent in the behavior of the rhesus monkeys, and even more so in the baboons, that Zuckerman had studied.[179] Hooton argued that captivity had introduced artificial conditions which had made these demonstrations extreme; however, the fact that C.R. Carpenter had observed similar types of behavior among rhesus monkeys in a wild (though not a native) habitat did, in Hooton's estimation, reinforce Zuckerman's position about their importance. Still, Hooton asserted, the rhesus-baboon pattern could not be generalized to all primates, for Carpenter had seen a very different style of social behavior among howler and spider monkeys -- e.g. infrequent acts of aggression, muted displays of dominance, and frequent instances of cooperative behavior between individuals.[180] Somewhat illogically, Hooton maintained that these interspecies variations in social behavior could be generalized to account for differences among human populations -- i.e. that they should give pause to "those who steadfastly adhere to the psychological unity of mankind, irrespective of marked physiological and anatomical differences such as are usually found among its main physical divisions."[181]

In apes, the information about sexuality and dominance was even less extensive than for monkeys, but in Hooton's view it showed promise of considerable interspecies variation as well. Gibbons had been studied in the wild by Carpenter, who had apparently found that dominance gradations did exist, but were secondary in their effect on social groupings to the strong antagonism that existed between individuals of the same sex and similar age. In chimpanzees, H.W. Nissen (1901-1958), who had done the major field study on the species, had not had an opportunity to assess social relationships in detail, so studies in captivity were Hooton's only useful sources. These studies suggested that both sexuality and dominance had less powerful roles in chimpanzee social behavior than among baboons, and that both categories of behavior were more smoothly integrated into the system of chimpanzee social relationships.

Indeed the impression of a nice balance was such that Hooton delivered the following summary judgement on chimpanzee society: "these animals are sufficiently intelligent to manage their group affairs peaceably and with a fair degree of independence and some measure of happiness for each individual member."[182] This judgement, he noted with irony, acquitted him of any taint of anthropomorphism in his evaluation of animal social behavior, given the contrast between it and the human world circa 1940. Chimpanzees, Hooton also noted, would have to stand for large-bodied apes in general, since there was as yet little information on gorilla or orang society either in the wild or in captivity.[183]

The other category of behavior about which a fair amount of information had been published by 1940 was territoriality, the discovery of which Hooton called perhaps the most significant finding that had been made in "primate sociology."[184] Not only had it been found present in a sizeable sample of species already, but it had also emerged as an important element in social behavior in every field study that had been done up to that time. In his view this marked territoriality as probably a "very ancient primate inheritance," one which had acted as an important causal factor in the differentiation of species by means of inbreeding and natural selection.[185] Territoriality, conceived as an "innate or acquired habit of relative immobility," seemed to Hooton to have been especially important in human evolution, for it provided just the sort of mechanism necessary to account for the "early differentiation of the very distinct physical varieties or races of man." The concept, he claimed, in a foreshadowing of the ideas of 1960s writers like Robert Ardrey, even had value for the study of modern human societies, for it rendered the frequent occurrence of wars touched off by the migration of peoples and the ensuing violent defense of already occupied territories biologically meaningful.[186]

While plausible on the surface, Hooton's interpretation of territoriality provided an interesting example of how new concepts could be placed in the service of old ones by subtle distortions of meaning. By defining territoriality as a "habit of relative immobility" he was really enlarging the boundaries of the concept, making it into a kind of self-enforced reproductive isolation of an entire breeding population or "race". However, even though primate territorial groups might indeed act to repel outsiders as a rule, this surely did not mean that there were barriers to gene flow into or out of such groups when they were forming, when they broke up, or when animals raised in one territory migrated in search of mates. As in his strained analogy concerning interspecific variations in dominance behavior, here too Hooton seemed to be using recent primate studies to support the notion that the race concept was still a biologically important one.

After his discussion of new themes in the study of primate societies, Hooton proceeded to recount the then existing state of research on the primate brain. The argument was entirely derivative in its concept of cerebral function, and strongly endorsed the theory of cortical localization; it thus added little to what Hooton had said about the brain in his earlier writings.[187] After this anticlimax, Hooton launched into his concluding remarks, in which he tried to characterize the overall evolutionary status and prospects of present day hominoids, and in particular to assess their relative degrees of specialization. He was especially interested in whether each species had become adapted so narrowly to a particular environment or style of life that its survival would be endangered by environmental change. As might be expected from the book's title, Hooton's prognosis was not encouraging, even if the pernicious influence of human actions were laid aside.

In regard to the pair of east Asian apes, Hooton believed that the gibbon had the better chance for survival. It was highly successful within its forest environment, and had been morphologically stable for a very long period of time. On the other hand, the gibbon's extreme brachiating specialization had apparently not had a good effect on the further evolution of that creature's brain, calling into question its ability to adapt to alterations in its habitat. The orang, in Hooton's view the most specialized of all the great apes, seemed to be in much greater trouble -- indeed, according to the punning prognosticator, it was "on its degenerate last legs."[188]

The pair of African apes presented a similar contrast between its larger and smaller species. The gorilla had in Hooton's view succumbed to a giantism that was usually the signal that an evolutionary dead end had been reached, while the mountain variety displayed an almost quadrupedal mode of adaptation to its environment, which reflected even more clearly an exhaustion of evolutionary solutions. The chimpanzee, however, seemed to him to be "the ape most likely to succeed," if any indeed were likely at all. Because its morphology made it a "better compromise between arboreal and terrestrial life" and its behavior, especially its capacity for rudimentary tool use, seemed more versatile than that of its non-human relatives, the chimpanzee seemed to him more likely to respond favorably to new environmental challenges and opportunities. Again, he asserted, Cope's principle of "survival of the (relatively) unspecialized" could be seen in operation.[189]

Though Man's Poor Relations did not analyze fossil primates in any detail, this consideration of evolutionary prospects provided Hooton with the opportunity to make a few observations about what he considered to be a puzzling group of "extinct apes." Though they were not specifically named, it is obvious from the content of his remarks that he was speaking about the australopithecines. Like most of his contemporaries he not only resisted classing the australopithecines as true hominids, but also believed that they had died out without leaving any progeny. Still, like many others as well, he recognized the significant hominid-like characters that they displayed. Thus, he felt the need to make the following comment about their fate:[190]

> It is rather discouraging to contemplate the undeniable fact that at least several of these [australopithecines] were more like man than the surviving anthropoids, and yet, by reason of their humanoid convergence, derived no benefit in the struggle for existence sufficient to keep them from dying out. As far as they were concerned, half-a-man seems to have been inferior to an all out ape.

Hooton should not only have been discouraged here, but probably unhappy with his own interpretation as well. The australopithecine case could have been seen as a clear contradiction of his principle of "survival of the unspecialized," for it could easily be argued that the "apes" in question were rather less than more specialized in comparison to the great apes by reason of their "humanoid convergence." And rather than to imply some mysterious cause for their extinction, it ought to have been easier and more consistent for him to posit the same mode of disappearance that he applied to other creatures that had shown supposed mixtures of hominid and anthropoid characters -- e.g. "Pithecanthropus," Piltdown and Neanderthal; that is, that they had succumbed to competition from more advanced hominids. Indeed, it seems reasonable to hypothesize that the ease with which many scientists, from Raymond Dart to Louis Leakey, accepted the idea of Australopithecus africanus as a hominid dead end reflects the weight of tradition in giving plausibility to this mode of extinction of "primitive" forms of man.[191] Hooton was apparently able to avoid falling into this familiar pattern of thought only because he was committed to another, and perhaps worse, old habit -- that of excluding small-brained animals from the hominid family on a priori grounds.

Up From the Ape -- Revisited

Inserting a couple of sentences about the australopithecines at the end of a work on primatology obviously did not suffice an an examination of the new issues that these and other fossil finds were raising in the paleoanthropological world of the early 1940's. Though the onset of World War II had curtailed exploration and led to the genuine scientific tragedy of the disappearance of the original specimens of Peking man,[192] the years 1939 - 1945 also saw the publication of two classic reports summing up major discoveries of the preceding decade -- McCown and Keith's volume on The Stone Age of Mt. Carmel and Weidenreich's monumental monograph on The Skull of Sinanthropus.[193] In addition, there were further finds of "Pithecanthropus" by Ralph von Koenigswald, the priliminary descriptions of which had been published by Weidenreich also.[194] Both the evidence and the theories advanced in these works posed serious questions for the interpretation of human evolution that Hooton had developed and defended since the late 1920s. Perhaps just as important, the war years saw the first attempt by a scientist working in the United States, namely Theodosious Dobzhansky (1900-1975), to apply the lessons of the neo-Darwinian "new synthesis" in evolutionary biology to the data on fossil hominids.[195] There were thus valid intellectual reasons for Hooton to rework his major opus on human evolution; the product of that labor was a new edition of Up From the Ape, which appeared in 1946.[196]

Despite all the fresh and brisk winds blowing across the anthropological landscape, Hooton's revision managed to retain most of the key features of the original version. This was most true with regard to comparative brain anatomy and comparative psychology, especially as he had gone over much of the same material in Man's Poor Relations. His accounts of cortical localization, and particularly of the brain mechanisms related to human speech, were almost identical to those contained in the first edition, reflecting the theoretical style of the 1920s without much change.[197] Hooton still found it necessary to quarrel with Elliot Smith's arboreal theory as well, making the same fundamental criticisms -- i.e. the theory's Lamarckian overtones, and above all, its inability to explain how the first hominids had come to differ in intelligence from "arboreal" apes such as the chimpanzee, so that hominids were able to adopt ground-dwelling habits successfully.[198]

As the last point above implies, Hooton continued to hold to the underlying assumption that had always shaped his response to the evidence -- that intelligence, conceived as a combination of "initiative" and the capacity to "profit by experience", had led the way over anatomical change in the emergence of humanity.[199] Though comparative psychologists had shown apes and even Cebus monkeys capable of "profiting by experience" to the point of learning to use tools (an issue which Hooton had emphasized in Man's Poor Relations [200]), he persisted, without supporting data, in granting hominids the edge over their relatives in both faculties at a point prior to the adoption of ground dwelling and bipedal locomotion. And as before, he argued that it was this edge that explained the hominid transition to a terrestrial habitat. "The great apes," he proclaimed metaphorically, "are 'die-hard' Tories; that is why they persisted in their leafy abodes. Our ancestors were Radicals; they 'took a chance' on the ground."[201] Also as before, in supporting this theory Hooton looked to a qualitative reading of the endocranial cast of "Pithecanthropus erectus" to show the great advances that hominids had supposedly made in brain organization by "Pliocene" times.[202]

The years since 1931 had not shaken Hooton's faith in the uniqueness of the hominids in brain size and complexity, or in intelligence. Another cornerstone of his earlier position -- the hypothesis of a Miocene, brachiating ancestor for the hominid line -- Hooton clearly wanted to believe in as much as ever, but he was finding it difficult. As we have seen,[203] the studies in comparative anatomy and paleontology done prior to 1930 by Keith, Gregory and Morton had provided the original support for this idea. Now Hooton was able to point to primate field studies as supplementary evidence. For example, he noted, C.R. Carpenter's studies of gibbons had shown these small-bodied apes to be almost totally arboreal, while the chimpanzees studied by H.W. Nissen had been observed to spend much of their time on the ground. Such data, Hooton argued, confirmed the hypothesis that relatively large body size was an important preadaptation for the adoption of ground dwelling habits by hominids.

Neither the requisite body size nor a brain sophisticated enough for habitual use of tools was in Hooton's opinion likely to have occurred before the radiation of the dryopithecine group in "middle Miocene time." Hooton even argued that the trend of recent archeology to rule out the authenticity of "eoliths" (of which he had been a strong supporter in 1931) seemed to bear out the theory of a relatively late emergence of the hominids from the anthropoid stock.[204]

A key piece of evidence was still missing, however -- sufficient post-cranial material from Miocene dryopithecines to determine their mode of progression conclusively. In the meantime, the "Keithian" argument from comparative anatomy, which in 1931 had seemed so convincing, had been eroded by the work of Adolph Schultz, William L. Straus and others. Hooton felt obliged to compare the evidence for and against a pongid-like ancestor point by point. His conclusion was a vague sort of compromise position -- the "proto-man" was less given to "cramping specialization" than the ancestors of the present pongid species, but he was nevertheless a "generalized tree ape" who "brachiated in moderation, walked the branches sometimes as a biped (as does the gibbon), and perhaps even on occasion went on all fours along the boughs." On his phylogenetic tree Hooton had the hominid, pongid and hylobatid lines all diverging from one another in the early Miocene, with the last mentioned line splitting off ever so slightly before the other two parted company.[205]

Miocene fossil evidence might still be sparse, but many of the geologically more recent remains which had accumulated since 1931 posed questions for Hooton's theories that he had to confront. He managed to do so without modifying his perennial position on Plio-Pleistocene hominid phylogeny -- that is, that "asymmetry" and "multilinearity" had ruled the process. In a logical sense, these two concepts were alsmot impervious to new evidence -- since no two fossils were exactly alike, by highlighting differences between new and existing specimens one could continue to sustain the picture of multilinear change, especially if, as Hooton did, one portrayed the "racial" differences found as non-functional in origin. Hooton, though, did not take this easy way out. Instead, he tried to integrate most of the recently discovered material into the various lines of evolution he had previously worked out. His studies on contemporary populations of Homo sapiens gave him the subsidiary hypothesis required to handle worrisome "intermediate" forms -- the phenomenon of racial hybridization. A mere glance at the hominid family tree that resulted (see figure 2) indicates the difficulties that attended this approach; complexity and confusion in explaining details are often the price paid for maintaining consistency in outmoded "paradigms," or even less comprehensive theories.[206]

There was, however, one major group of details that Hooton refused to work into his scheme of hominid evolution -- the australopithecines. By this time the world had long become aware that the "Taungs baby" was no aberration. Similar fossils had been collected by Robert Broom in the late 1930s and early 1940s, and Gregory had already made a very strong case for the hominid character of the australopithecine dentition. The most detailed debates over the "man-apes" were only beginning, however; Broom's fossils were described fully only in the year that Hooton's book appeared. The turning of the interpretive tide in England was set off by W.E. LeGros Clark's "conversion" in 1947.[207] The irony of Hooton's treatment of the australopithecines was that he chose to

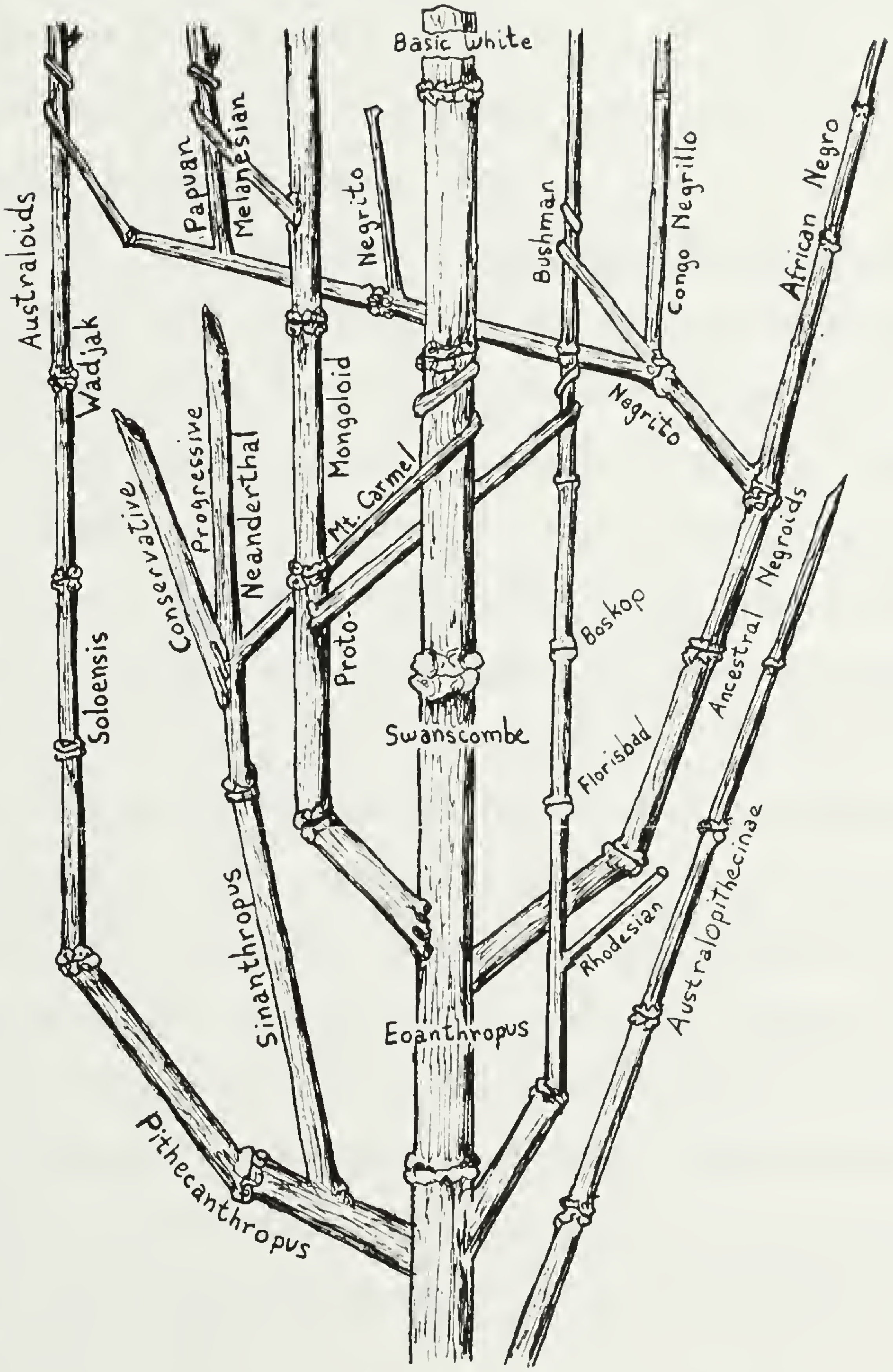

Figure 2. Phylogenetic relationships among known hominid "types," according to E.A. Hooton. After Hooton, Up From the Ape, 1946 revised edition, p. 413.

perpetuate a conservative interpretation of their significance while American opinion,[208] and even that of his mentor Keith, was shifting toward the inclusion of them in the human family tree.

The "type specimen" of Australopithecus africanus, the Taungs fossil, was still the most thoroughly studied of the "man-apes," and Hooton's conclusions about it were clearly derived from the consensus of earlier opinions about this fossil. First, Hooton noted, juvenile apes are always more manlike in skull form than adults of the same species, and Dart had not made sufficient allowance for that fact when he deduced the existence of gracile skulls and upright posture in australopithecine adults from the Taungs fossil.[209] Regarding the Taungs endocranial cast Hooton felt that he could accept Dart's original "speculative" conclusions more easily; because of the "lateral compression and increased height" of the endocast as compared with typical ape brains, there could, he thought, be "no doubt that the cerebral development of this fossil ape was progressive in a humanoid direction."[210]

Still, even after adding the endocranial evidence to what he agreed with Gregory were the "manlike" teeth of the australopithecines, Hooton was unwilling to call them hominids. The australopithecines were of Pleistocene date, he argued, and yet they undoubtedly lacked "the brain overgrowth that is specifically human and perhaps should be the ultimate criterion of a direct ancestral relationship to man of a Pliocene precursor."[211] Thus, for him, continued belief in the ancient appearance of large brains among hominids ruled out moderate brains in the group, even brains that he himself believed more "humanoid" than those of any other known ape, primitive or modern. If Hooton objected only to "direct ancestral" status for Australopithecus africanus on this basis, his argument would not have been unusual, even by modern standards, for some modern anthropologists make the small brain size of A. africanus as compared to its near contemporary Homo habilis an objection to putting the former in the direct line of human evolution.[212] Hooton, though, went beyond this, concluding that "because they lacked brains, they [australopithecines] remained apes, in spite of their humanoid teeth."[213]

A few of the reasons why Hooton was so quick to reject the idea of relatively small-brained hominids are clear. For one thing, it called into question his general conception of human emergence, in which intelligence played the leading role; this conception was, as we have seen, very congenial to Hooton ideologically, as well as deeply rooted in English and American paleoanthropology.

Hooton was also inclined to rule out the australopithecines because his interpretation of the fossil evidence had already given him "Pliocene precursors" more humanlike in brain size -- namely, Piltdown man and "Pithecanthropus."

It would be superfluous to analyze in detail the continuing importance of the Piltdown specimens in Hooton's picture of human evolution. All that need be said is that his faith in them was undiminished in 1946, and that he tried hard to transform what he discerned as the two principal challenges to Piltdown's importance into supporting evidence. Interestingly, the name of Franz Weidenreich, the most eminent paleoanthropologist then working in America as well the foremost critic of the Piltdown "fossils," was associated with both challenges. The first challenge was Weidenreich's flat assertion that the jaw of "Eoanthropus" was that of an orang; the second was the discovery, confirmed by Weidenreich's meticulous research, that the mandibles of both "Pithecanthropus erectus" and "Sinanthropus pekinensis" were much more humanlike, especially in tooth form, than that of the Piltdown creature.[214]

Hooton attempted to turn these objections around simply by invoking the phenomenon of "asymmetry." Suppose, he conjectured, that "some nearly human and fully human types of early man paralleled the orang in teeth, brow ridges, forehead conformation, and concavity of the middle face, whereas others resembled rather the chimpanzeee-gorilla line with supraorbital tori, fleeting frontal regions and mid-facial prognathism." Piltdown, of course, would have represented a large brained (1350 cc. cranial capacity in Keith's most recent estimate) member of the former group, while the Asian fossils would have represented members of the latter group varying in brain size. If this had been the case, he contended, the association of a "humanoid" braincase and an "apelike" jaw in Piltdown would be quite analogous to the reverse association in "Pithecanthropus" and "Sinanthropus", and no more unusual.[215]

These generalizations, almost identical to the way in which he had earlier handled the contrast bewtween Piltdown and European fossils like Heidelberg and Neanderthal man,[216] sounded reasonable as phrased by Hooton, but the concepts that lay behind them were problematical. As in his earlier discussions of the Piltdown issue, he gave no clue as to what functional basis these large differences between various hominid lines in skull form would have had. Another unsolved puzzle was why an earlier hominid which presumably began with closer relationships either to the African pongids or the orang had later produced descendants converging on the skull form of a distant relative with a very different mode of life. Also, while "Sinanthropus" skulls might resemble the chimpanzee-gorila form more than modern humans did, Weidenreich had made a strong case that the Piltdown mandible was indistinguishable from that of an orang; in addition, it was still not known how the Piltdown jaw could have articulated smoothly with the rest of the cranium.

Finally, there were problems relating to the number of fossils and the plausibility of the morphological judgements based upon them. By 1946, there was still only one reasonably "complete" skull and small fragments of a supposed second individual from Plitdown, and nothing new had been found for thirty years; by the same year about a score of individuals representing Java and Peking man had come to light, and those from Choukoutien had been subjected to exhaustive morphological study by Weidenreich.[217] To treat morphological generalizations about "Eoanthropus" and *Homo erectus* as equally well founded was dubious practice.

In light of these problems it would be easy to deliver a harsh judgement about Hooton's defense of Piltdown man, but it is more important to understand how he came to it and why it seemed sufficient for him.

First, there were broad traditions of interpretation behind the reasoning Hooton employed. The tenacity of his support for "Eoanthropus" resulted in part from his close ties to English paleoanthropology -- support for what was often called "The earliest Englishman (or woman)" had become an article of faith for an entire community, and it was hard to relinquish it. Also, the functional problems in the way of his explanation would not have worried Hooton greatly because, as we have noted so often, he was sceptical of functional interpretations of fossil morphology; indeed, it was truly difficult to infer function at the time he was writing, given the lack of comprehensive and precise data about primate functional anatomy or about the past environments in which hominids had evolved.

More general theoretical concerns were a factor as well. In 1946 Hooton still held to what he felt was a solidly based distrust of functional explanations of the differences between modern human races. If heritable, but non-adaptive features were the key to understanding the differentiation of modern races, perhaps the same approach would not lead one far astray in the interpretation of fossil "races." Finally, and probably most important, was the related influence of "typological thinking," which, as noted elsewhere, was a standard feature of the anthropology of Hooton's day.[218] Single specimens were deeemed sufficient to define "types", and once defined, all types seemed to have equal scientific weight. Once one got into the habit of comparing the "Piltdown type" with the "Sinanthropus type," one tended to forget the relative strength of the actual data base on which each "type" rested. Both had become reified, and new discoveries of one "type" did little to undermine one's belief in the reality of another.

In order to understand Hooton's position it is also important to remember that the 1930s had produced a crucial piece of evidence to buttress Anglophile pride in Piltdown man -- i.e. the Swanscombe skull. Since Swanscombe lacked a mandible, and the dimensions of the occipital portion of the skull were commonly deemed to be within the range of modern <u>Homo sapiens.</u> it could be made to stand as evidence both for the existence of "true men" in the mid-Pleistocene, and for the continuation of the "non-gorilloid" line begun by Piltdown. Both were claims that Hooton found reasonable; while his acceptance of the latter was qualified (so strong apparently was his belief in multilinearity), his enthusiasm for the former was very strong still.[219]

Despite the neutral overtones of the idea of asymmetry, Hooton's discussion of the most "primitive" fossils clearly revealed that he found the supposedly "smooth-browed," European varieties of humanity closer to the main line that produced modern humans than the "ape-men" of the Far East. As one might expect, the shape of the brain case counted for more than other characters. The way he handled the various members of the "gorilloid" line was thus very similar to that adopted in his earlier works, despite the tremendous accumulation of new evidence about them that had accumulated since 1930.

"Pithecanthropus" was the earliest of these "gorilloid" forms in time, and as far as Hooton's interpretation was concerned, the trend setter for the entire group. In his own analysis of the creature Hooton made use of Weidenreich's description of the overall skull form of "Pithecanthropus" as well as the reconstruction of it that had appeared in <u>The Skull of Sinanthropus Pekinensis,</u> but Hooton's judgements on the endocranial cast and assessment of the place of Java man in human phylogeny were little changed from similar passages in his earlier works. The conclusions on the endocranial cast were essentially as follows: the "Pithecanthropus" brain revealed syntactic ability, "handedness," plus sufficient expansion of the capacity for voluntary movement to allow for the freedom of the hand from locomotion and for tool-use; nevertheless, the small size of the parietal and frontal association areas indicated that these abilities fell far short of modern levels. Indeed, Hooton continued to believe that the behavioral capacities of "Pithecanthropus" fell so far short that, in spite of the generally accepted Pleistocene date for the existing fossils, Java man probably represented the "late survival" of an "archaic type that must have come into being at least in the Middle or Upper Pliocene."[220]

How, he asked, had this survival been possible? Again, he gave his standard answer: Java was a "refuge area," isolated from the main continental centers, and thus a place where "outmoded fauna" could escape competition with more advanced forms.[221] Rather than accept what is now known as _Homo erectus_ as a legitimate representative of the human family of the early Pleistocene, Hooton preferred to speculate that "an anatomically advanced and virtually modern form of man may well have existed when the apish _Pithecanthropus_ still reigned in Java."[222] Why was he so stubborn in maintaining this interpretation? The reasons were probably the same as before; once adopted, the habit of adjusting average dates to make the fossil record conform to expectations based on morphology died hard, and in addition, Hooton already had a Middle Pleistocene representative of modern _Homo sapiens,_ he believed, in

Swanscombe.

What of the most extensive and dramatic set of Middle Pleistocene fossils that Hooton had to confront -- those of Peking man? Here also he relied on Weidenreich's descriptions while trying to turn them into grist for his own mill. He agreed fully with the German scientist that the Choukoutien fossils were closely related to Java man, but would not accept Weidenreich's contention that they were a racial variant of roughly the same stage along the main line of human evolution as that represented by "Pithecanthropus erectus." There were, Hooton noted, several important characters in "Sinanthropus" which had been identified by Weidenreich himself, especially the average size of the brain, that were more advanced than those encountered in "Pithecanthropus" specimens. To him this indicated that Peking man was merely a slightly more modern representative of the so-called "gorilloid" line of hominids than Java man.[223] Thus, "Sinanthropus" added no really new "type" to the human family tree, and actually lent support to the idea of multilinearity, because (as noted above[224]) he saw its Middle Pleistocene date as contemporaneous with the more modern-looking Swanscombe skull. Peking man appeared also to him to have added further confirmation to the theory of asymmetry, since its straight and "fully human" thighbone contrasted greatly with its "apish skull cap."[225]

Hooton also continued to maintain that two other "gorilloid" forms of as yet undetermined age supported the multilinear picture outlined above -- Solo man and Rhodesian man. According to Weidenreich, the Solo fossils had strong affinities with "Pithecanthropus," while their enlarged cranial capacity and other details of their skull form indicated some advance toward "Neanderthaloid" status.[226] This evaluation, Hooton thought, supported his own notion that the heavy-browed "palaeanthropic" (in Elliot Smith's terminology) forms of humanity all bore close genetic relationships with one another. As it had earlier, Rhodesian man still appeared to be a particularly strong example of "asymmetry;" its upper jaw and palate were massive while its teeth were allegedly typically human in form. It also had, in his view, an exceedingly primitive supraorbital torus, yet the forward position of the foramen magnum (the area where the spinal cord enters the braincase) and the form of the thigh bone found with the skull indicated a posture more modern than that of the European Neanderthals.[227]

As we can see in the two preceding examples, evidence about posture was important to Hooton's case of "asymmetry". Of course, the expectation that "apish" skulls and "human" leg bones were not the norm for fossil hominids, but existed only in some "types," was based in large part on Boule's influential interpretation of Neanderthal man, which Hooton had long accepted. It was becoming clearer as the years progressed, however, that Boule had not said the last word on the subject of the Neanderthals. On this issue as on others, Hooton had difficulty squaring recent discoveries with received theories, but still he elected not to question the traditional wisdom. Instead, he only strove to make the application of that wisdom a bit less sweeping.

The central "new" evidence on the Neanderthal question continued to be the Mt. Carmel population; in addition, there was now a pair of "progressive Neanderthaloid" skulls from Germany, the Ehringsdorf and Steinheim fossils. Hooton recognized that none of these fossils, with the exception of the Tabun cave skeleton from Mt. Carmel, fitted the "classic" pattern of Neanderthal features. Though both had robust supraorbital tori, the Steinheim and Ehringsdorf skulls also seemed to possess higher skull vaults and more rounded foreheads than other Neanderthals, and Steinheim diverged further by lacking the supposedly typical Neanderthal "bun-shaped" occiput.[228] The Skhul cave fossils from Mt. Carmel, Hooton recognized, departed even more dramatically from the typical Neanderthals -- displaying much reduced facial superstructures, shorter jaws, well-rounded occiputs, and much higher skull vaults and foreheads.[229]

A ready solution was at hand, however, for these apparent difficulties, and one that would save the hypothesis that there had been an apish, evolutionarily stagnant Neanderthal "type." Why, Hooton queried, could there not have been two lines of Neanderthaloids: a "conservative" type which had "become set in the classic mold of the man of La Chapelle, and a progressive, continually evolving type, exemplified by Steinheim, Ehringsdorf, etc."[230] The fact that both the "progressive" forms mentioned were at the time Hooton was writing generally considered earlier in date than their "conservative" relatives he did not confront directly (though an invocation of type of "morphological dating" commonly applied to "Pithecanthropus" could easily have solved the difficulty).

Once he had raised the possibility that some Neanderthaloids had evolved in a progressive direction, Hooton was under some obligation to indicate how far these forms had evolved. The belief of Hrdlicka and Weidenreich that progressive Neanderthaloid populations were directly ancestral to modern Homo sapiens went too far, he thought, but the Neanderthaloid group might "well have produced, all of itself, some such archaic form of Homo sapiens as the Australian aboriginal." Other, and by implication less archaic forms of Homo sapiens, he continued to maintain, had probably evolved "through protohuman types that never carried the chimpanzee-gorilloid supraorbital torus."[231] Though this formulation left the door open for genetic contributions to modern humans by both "progressive Neanderthaloids" and smooth-browed early Homo sapiens, it clearly implied that the latter group was mainly responsible for the genetic makeup of "civilized" populations of modern humankind.

Though Hooton's account conserved the essentials of the view of the "classic" Neanderthals established by Boule, Keith and Osborn, he differed with the older theorists in the way he conceived of the replacement of the Neanderthals by more advanced types. Thus, he did not argue that the Neanderthals had disappeared because they had been exterminated or starved out by the superior "Cro-Magnons;" instead, he asserted that the Neanderthal morphological type had disappeared because the genes that produced it had been "absorbed and swamped by admixtures with progressive and genetically dominant types of Homo sapiens."[232] Again, Hooton was using the language of modern genetics to explain an evolutionary scenario that violated the spirit of the new systematics based on the new genetics, a scenario that was really founded on an outmoded scheme of racial typologies.

The last piece in the Neanderthal puzzle was the evidence from Mt. Carmel, and Hooton made a comfortable fit for it as well -- by categorizing the Skhul specimens as examples of hybridization between Neanderthaloids and "neanthropic" populations. The alternative view that Skhul revealed the Neanderthaloids in the process of evolving into modern Homo sapiens he did not find persuasive, as he had not when it was first suggested in the late 1930s. Even more unacceptable was the final theory of Keith and McCown that the Mt.Carmel people were a genetically unstable population occupying a transitional zone between an eastern region where "neanthropic" forms were evolving and a western "paleanthropic" region. Hooton rejected both ideas because he believed that they failed to "recognize" the likelihood that "neanthropic" line had already produced populations of modern Homo sapiens in western Europe well before the appearance of the population represented at Skhul. In addition, the McCown-Keith thesis seemed to him to have rendered Palestine "a sort of evolutionary no-man's land in which the genes and characters of one form and the other vacillated in the germ plasm and skeletal structure of the individuals therein resident." It made much more sense biologically, he thought, to conceive of the transitional zones between centers of "neanthropic" and "paleanthropic" races as zones of "intermixture and hybridization," zones in which composite morphological types had resulted.[233]

While considerably more subtle than the picture of Neanderthal replacement promoted and popularized by Osborn or MacCurdy, Hooton's picture of the Neanderthaloids and their place in the emergence of modern Homo sapiens rested on a somewhat capricious use of biological theory. In his stress upon the idea of hybridization and his conception of Mt. Carmel as part of a "zone of intermixture" he was clearly trying to apply some of the lessons of modern genetics to paleoanthropology, and to excise the crude (and generally racist) analogies with modern racial conflicts that purported to explain the later stages of human evolution. At the same time, though, Hooton's scenario flew in the face of the newer ideas about speciation that were allowing geneticists like Dobzhansky to question the whole tradition in paleoanthropology in which Hooton was working.

Why, for example, should one believe that there had been multiple lines of hominid evolution without evidence of differing types of ecological specializations among hominids? How could lines that had been reproductively isolated long enough to have formed different species and even genera have hybridized so easily? And how could the "races" within a single species, Homo sapiens, be, even in part, the offspring of ancestors from two or more species? Hooton's account raised all of these issues; the fact that he failed to discuss any of them demonstrates that he had not really begun to incorporate the basic concepts of the "new synthesis" into his thinking, though he was willing to appropriate some of its language.

Still, one cannot not judge Hooton too harshly in this regard, for the new ideas about species had not been uniformly accepted even among leading proponents of Mendelian genetics. An important example was Hooton's Harvard colleague, R.R. Gates (1882-1962), who published his own comprehensive review of human evolution in 1948, adopting what he called a "genetical point of view," and hypothesized the existence of five separate species of anatomically modern humanity evolving in parallel with each other. That Hooton and Gates had great respect for each other's work is evident from the fact that Hooton wrote a laudatory preface for Gates' book.[234]

Conclusion -- "Twilight of the Idols"

In sum, then, the revised version of Up From the Ape, Hooton's last major work on human evolution, showed him to be a fundamentally conservative, but not a reactionary figure. He did not ignore new findings or new ideas, and generally did not attempt to bury them under adverse criticism. Still, his maximum effort went into contriving explanations that would make the new data fit comfortably into the evolutionary scheme and style of reasoning to which he had committed himself long before. Though the influence of Keith especially can be seen in the early formulations of Hooton's perspective, by 1946 the latter had made it unquestionably his own, and had spent a great deal of energy elaborating and defending what he had produced.

A major symbolic test of Hooton's commitment to his own conception of human evolution came rather soon after the publication of the revised Up From the Ape -- in the form of a new book by Sir Arthur Keith, and entitled A New Theory of Human Evolution.[235] Published in 1948, this work summed up a surprising shift in the octogenarian's views on fossil hominids, a shift that had been in progress since the time Keith had been working on the Mt. Carmel fossils.[236] The man who had perhaps done most in the English-speaking world to keep the Neanderthals on a side-branch of the human family tree and to question the hominid status of the australopithecines, was now welcoming both as full fledged human ancestors.

Hooton's only published comments on Keith's final desertion to the enemy camp came in a brief review that the former wrote for the English periodical Antiquity. The tone of the review was respectful; Hooton took time to remind his readers of Keith's pioneering accomplishments in the study of primate anatomy, and to endorse the scepticism about overcoming group conflict in human affairs that was a major theme of the book. Indeed, both writers agreed that competition between and among genetically isolated human "racial" groups had been a major engine of hominid evolution.

Hooton's major criticism of Keith's "new theory" came in regard to the crucial point that all the former's previous discussions about asymmetry and multilinearity had striven to demonstrate -- i.e. the great age of anatomically modern forms of man. "Perhaps most interesting to the Physical Anthropologist," Hooton suggested with considerable irony and understatement,[237]

> is Sir Arthur's abandonment of his position as the champion of the early Pleistocene existence of anatomically modern man (Homo sapiens) and his acceptance of the belief held by Hrdlička, Weidenreich and others that modern man is a direct descendant of such apelike Pleistocene human forms as Neanderthal man. This apostasy comes at a time when the discovery of Swanscombe man, and Fontechevade man (the latter subsequent to Sir Arthur's book) seems finally to confirm the correctness of this earlier view.

This review was Hooton's last published word on the later stages of human evolution, and it makes clear his own "Tory" status on matters of interpretation. For him there would be no autumnal reversal of views as there had been for Keith, and after the review of Keith's book he wrote only a single brief piece dealing directly with human phylogeny. Perhaps his silence on the issue had something to do with a sense of isolation. It is difficult enough to be a "Tory" during a revolutionary era; when the war is ending and even one's "prime minister" begins to reveal "Radical" sympathies perhaps silence is indeed the best course of action. Without looking into Hooton's personal papers, though, the reasons for his reticence can only be guessed at.

Whatever his reasons, Hooton made only one contribution to the debate over human origins after 1949, on an issue that he could hardly have ignored -- the uncovering of the Piltdown hoax. He registered his reaction to what had surely been a sad event for him in a letter to the editor of the American Anthropologist. The specific occasion for the letter was a short article by Sherwood Washburn on the Piltdown affair that the journal had published in 1953. Washburn had mainly summarized the findings of various tests made on the Piltdown remains by the English scientists J.S. Weiner, Kenneth Oakley

(1911-1981), and Wilfred E. LeGros Clark (1895-1971), but had also added a few sentences indicating his views about the lessons to be learned from the hoax.[238]

Central for Washburn had been a pair of methodological principles that he believed had too often been ignored in writings on the Piltdown problem in the past. The first was that theories about hominid phylogeny and relationships should be erected only on the basis of well-preserved and securely dated skulls that contained sizable and unreconstructed portions of braincase, face and jaw. "The greatest lesson of Piltdown for the student of human evolution," Washburn counseled, "is that <u>there was never enough of the fossil to justify the theories built around it</u> [Washburn's emphasis]."[239] The second methodological lesson he drew was that theories could never be considered well-founded unless they had been arrived at through "studying the originals with all the techniques available."[240]

The discovery, delayed too long by limited access to the originals, that Piltdown had been a fake would, Washburn believed, mark "the end of an era" in which these two principles could be ignored because of the rarity and preciousness of the few human fossils that existed.[241] Finally, toward the end of his notice, Washburn suggested what effect the removal of Piltdown man might have on conceptions of human phylogeny -- i.e. that the exclusion of a fossil commonly alleged to be an early representative or near representative of Homo sapiens would "strengthen the general theory of human evolution outlined by Weidenreich in which Homo sapiens appears very late."[242]

In his own comment on the Piltdown issue Hooton chose to address two of Washburn's three key points, but he also described his personal reactions to the "affair" as well. He confessed that it had been very hard for him to accept the idea that a fraud had been perpetrated, for he had known Sir Arthur Smith Woodward, the principal describer and the custodian of the fossils, and could never believe the latter would involve himself in a hoax. Though he had not known Charles Dawson, the discoverer of the Piltdown remains, Hooton had difficulty crediting the idea that Dawson had been the hoaxer, either.[243] Indeed, he felt that it involved greater strain on his credulity to accept the conclusion of deliberate fraud than it had been "to believe in the legitimate association of an apelike mandible and a completely human brain case in the same individual."[244] This association had never seemed a matter of "swallowing a camel" for him ,he explained, because the phenomenon of "asymmetry" was in his view "strikingly manifest" in the evolution of humanity. The Piltdown hoax had evidently done nothing to shake Hooton's belief in this principle, for he took the opportunity to include another illustration of "asymmetry" in his letter, namely the fact that "the dentitions of the Australopithecines seem more humanoid ... than one would guess them to be on the basis of size and morphology of the associated brain cases."[245]

Hard as it was, and still harboring a faint hope that the "ebb and flow" of scientific currents might restore at least part of the Piltdown material to the status of genuine human fossils, Hooton recognized that he would have to "eat crow." The evidence as it existed made the hypothesis of fraud the best available one. In accepting that evidence he expressed his fear that Piltdown would become grist for the mill of antievolutionists, that the discovery of "calculated dishonesty" in the ranks of evolutionary biologists would tarnish the discipline as a whole in the eyes of an all too sceptical public.[246]

While he accepted the findings of the trio of British scientists, Hooton made it clear that he would not accept the conclusion Washburn had drawn from the episode -- that there had never been "enough of the fossil" in the first place. "I do not agree," he asserted, " that anthropologists should refrain from formulating theories of human evolution around incomplete and fragmentary fossils." If Eugene Dubois had held back, he argued, from theorizing about "Pithecanthropus" because he only possessed "the calva and the supposedly associated femur" of the creature, an "important phase" of human evolution would not have been discussed adequately until Ralph von Koenigswald's discoveries fifty years later. Similarly, if Raymond Dart had been hesitant, would Robert Broom have been stimulated to uncover more data on the australopithecines?[247]

To Hooton the "great lesson" of the affair was not to refrain from theorizing but to be willing to admit that no "proofs" of a theory are final. "Anthropologists," he proclaimed,[248]

> need not be rash and irresponsible in the interpretation of fragmentary evidence, but they should not be pusillanimous and motivated principally by caution and fear of being proved wrong by future discoveries. It has always seemed to me that the persons who in science or in any field of thought stand in perpetual fear of being "wrong" are never really right.

Hooton also took the time to reject Washburn's inference that the removal of Piltdown confirmed the theory of the late appearance of <u>Homo sapiens</u>; in his view "no radical readjustment" of theories of human emergence need take place. It was "still possible and wholly probable," he claimed, that early Pleistocene <u>Homo sapiens</u>

(in as complete a form as Washburn might demand) would still be uncovered. Indeed, Hooton underscored his refusal to recant with the following words: "the present writer, who may be gullible to the extent of perversity, would not be suprised to live to witness the discovery even of an authentic Eoanthropus -- jaw, brain case, and all."[249] Both criticisms of Washburn made Hooton's response to the Piltdown affair abundantly clear, but they also revealed a contradiction in that respone as well -- for did not his stubborn insistence on the early sapiens, and even the "Eoanthropus" theory in the face of all that had happened imply that he was really unable to live up to the lesson about intellectual flexibility that he had supposedly learned?

The last piece of Hooton's that had any relevance for the problem of human evolution appeared posthumously, in 1954. It was an essay calling for increased emphasis on studies of living primates, and a claim for their great importance in understanding critical issues in physical anthropology.[250] The essay was also typical of Hooton in the way that it combined genuine openness to new research methods, scepticism of the "sacred cows" supposedly venerated by other physical anthropologists, and vigorous promotion of his own point of view.

In the course of his discussion Hooton devoted a passage to the problems of interpreting fossil hominids and reconstructing human phylogeny. His least controversial point involved the following rhetorical question: could anthropologists truly make plausible reconstructions and convincing phylogenies for primitive fossil forms such as the australopithecines, when definitive studies of skeletal variation in such familiar modern species as the chimpanzee were still lacking?[251] Another problem regarding what he called the "ever intriguing Australopithecinae" involved the relation between the brain and tool use. Possibly, he conceded, Dart's theory that the australopithecines had possessed an "osteodontokeratic" culture made sense, but it was clear that further study of primate brains would be necessary in order to find out "how large a brain, or how complicated a nervous organization, is required to effect the transition between using natural objects and fabricating or trying to make tools and weapons."[252] While these remarks showed that Hooton was keeping up with what was then a "hot" topic of conversation in paleoanthropology, they also implied a continuation of his reluctance to grant the australopithecines clear hominid status. Perhaps, he seemed to be saying, further studies of non-human primates would uncover analogies between the morphology and supposed behavior of the australopithecines and those exhibited by living pongids.

Ever the controversialist, Hooton preached the need for primate studies because he claimed that the study of contemporary non-literate peoples had revealed, and probably could reveal, little about the earlier stages of human evolution. From the physical standpoint, he said, there were "no stages of human evolution discernible in the anatomy and physiology of recent man; only variations, mosaics of progressive and retrogressive characters."[253] The first part of the statement was one that most anthropologists of the post-World War II era would have accepted; the second part, though, clearly harked back to earlier traditions, and indeed to his first article on the "asymmetry of human evolution."

Even more foreign to the spirit of the postwar era were some observations Hooton made on the study of behavior. More could be learned, he claimed, about the beginnings of human family and social organization from studies of *infra* -human primates in their natural habitats than "by the study of retarded human groups living today under conditions variously described as 'primitive', 'uncivilized', or 'savage'." Though a bit testy, such a statement would not, except for the use of the word "retarded," have been hard to defend at the time, since it stressed concepts like human behavioral kinship with other primates,and the continuity of anthropology with zoology. Indeed, it seemed to mark a change of emphasis from the days when Hooton stressed the uniqueness of early hominid "initiative."

When one notes how Hooton supported his statement, however, one sees that things had not changed so much at all, at least from the Hooton of the late 1930s. The study of "retarded" peoples was unfruitful because, he contended,[254]

> these contemporary savages are not "primitive," not on the evolutionary upgrade, not the stuff of which societal progress is made. Whether environmentally underprivileged or genetically underendowed or both they are cultural imbeciles or morons -- at any rate if we believe our "civilization" is superior to their rude way of life.

Though they did not constitute a self-conscious attempt to "sum up" on Hooton's part, these final remarks on evolutionary questions will have to serve that purpose here. Whether Hooton would have continued (like his "poor relation" the gorilla) to be a "die-hard Tory," or whether he would have eventually retreated from his ideas about "asymmetry," multiple lines of hominid evolution, and the early appearance of Homo sapiens, had he lived longer into the postwar era, is unclear. What is clear is that echoes of old-fashioned ethnocentrism and racial determinism sounded strongly in his words about modern "primitives."

It is also evident that these echoes served to create a certain discordance which we have seen displayed throughout all of Hooton's later work -- the clash between a professional seeking to broaden the boundaries and the impact of his discipline, and a parochial theorist unable to grow beyond the constricted intellectual assumptions acquired in youth. Both intellectual styles were present in Hooton, and as physical anthropology changed they had come increasingly into conflict with each other. In estimating Hooton's final place in the history of the discipline, and particularly of the study of human evolution, neither can be forgotten. Otherwise it would be difficult to explain how a teacher who was an inspiration to and a liberating influence on his students, a methodological innovator, and a writer who embodied the science of physical anthropology for a generation, could also have left so little in the realm of theory and interpretation that later workers could build on.

CHAPTER V

WILLIAM KING GREGORY, 1876 - 1970

Gregory's Life in Brief

William King Gregory, for many years curator of vertebrate paleontology at the American Museum of Natural History as well as DaCosta Professor of Zoology at Columbia University, was a native New Yorker, born in that city on May 19, 1876. His father, George Gregory was a printer, and William spent his early childhood years in lower Manhattan, living, as E.H. Colbert notes, "in the upper and rear part of a small house, the front of which was occupied by his father's printing shop."[1] Young Gregory attended St. Luke's Primary School, and then public school for a few years; he finished his college preparatory studies in the "science course" at the Trinity School in New York, after which he enrolled in the Columbia School of Mines.

While at Columbia Gregory's interest eventually shifted from applied to pure science, specifically the science of zoology. His first mentor in the field was Bashford Dean (1867-1928), who inspired in Gregory a lifelong interest in the study of fossil fishes, his own research specialty. After he had transferred to Columbia

College as a major in zoology and vertebrate paleontology, Gregory came under the influence of Henry Fairfield Osborn, at that time the nation's leading expert on fossil mammals. Though still an undergraduate, Gregory became Osborn's reseach assistant and demonstrator in the fall of 1899. After getting his B.A. in 1900, he remained at Columbia, doing his graduate work under Osborn's direction.[2]

Gregory received his doctorate from Columbia in 1910, and in the following year was appointed to the scientific staff at the American Museum of Natural History. He eventually rose to become full curator there, in the department of vertebrate paleontology; for a long time he worked simultaneously in that department as well as in those of comparative anatomy and icthyology. Along with his research activities, he was also instrumental in putting together what in his day were two of the museum's principal exhibit halls -- the "Hall of Fishes" and the "Hall of Comparative Anatomy." In addition, he found time in his early years at the museum to serve as editor of the fledgling American Museum Journal, which would eventually grow into the magazine Natural History . In 1916, Gregory became a faculty member in the Zoology Department at Columbia, where he would remain for the rest of his teaching career. In this role, and with the aid of the

American Museum's excellent research collections, he trained, in Colbert's words, "a large contingent of able vertebrate paleontologists and zoologists, including many of the leaders of these fields in North America."[3]

Gregory apparently possessed admirable qualities both as a scientist and as a person. As Sherwood L. Washburn has pointed out, Gregory had a "remarkable ability to keep a number of major projects going at the same time, and, frequently, papers on mammals, fishes and reptiles all appeared in the same year."[4] The way in which he divided his interests did not seem to diminish the quality of his production, for he made important scientific contributions in several areas of study -- including, but not limited to the evolution of mammal-like reptiles, both fossil and present day fishes, and primates and other mammalian groups. Though the study of the dentition was the area of specialization in which he received his greatest recognition, he was able to achieve so much in his work on past and present vertebrates because he had developed a broad and yet extremely detailed knowledge of their "functional anatomy" -- the way various parts of the skeleton came together to form functioning, adaptive structures.[5]

Another hallmark of Gregory's overall scientific achievement was his ability to enter into fruitful, and in several cases, very long-lived collaborations with other scientists. Over the years he worked with contemporaries, like Henry C. Raven (1889-1944) in the field of comparative anatomy and Milo Hellman (1872-1947) in the study of higher primate dentition, with younger scholars, such as A.S. Romer in studies on extinct tetrapods, and of course with his teacher, Henry Fairfield Osborn. The last case mentioned is an expecially interesting one, for in the long years first as Osborn's assistant, and later as his junior colleague, Gregory compiled much of the detailed research that Osborn included in such works as The Age of Mammals, and his "gigantic" monographs on the proboscideans and extinct titanotheres.

In these years Gregory also came into prominence as a scientist in his own right, and a scientist who did not always agree with his distinguished teacher. That this remained a "close and friendly relationship, enjoyed by both parties" and that Gregory was able to maintain his intellectual independence at the same time was a great achievement on Gregory's part, for as Colbert points out,[6]

> Osborn was not an easy man to work with; he was demanding and imperious. Moreover, he did not like to be disputed. But Gregory handled him with remarkable finesse, so that when they were poles apart, as for example on the subject of primate evolution and the origin of man, there were no hard feelings. To Osborn, Gregory was his "fidus Achates," to Gregory, Osborn was his "imperial

mammoth."

That Gregory did not have to sacrifice his own views in order to maintain his amiable relationship will indeed become clear when we look at the debate that the pair conducted on the issue of human evolution in the late 1920s.[7]

There are some personal qualities of Gregory's that help perhaps to explain both his successful collaborations with others, and to some degree, his overall scientific achievements. Colbert, who knew Gregory well, describes him as "a truly delightful person. He was quiet, he was modest, he was sincere. Perhaps one of his outstanding characteristics was his enthusiasm for life and for the world around him. Indeed, the living world had for him the fresh delight that it has for a child."[8] It could be that this never failing enthusiasm for the variety and richness of the natural world was what motivated him to pursue the study of so many of its various manifestations. It also seems to have played a great role in shaping his desire to place the human race in its natural context as a product of the evolutionary forces that had shaped the rest of the living world.

Though not a full-time worker in the field of physical anthropology, Gregory had great importance as a student of human evolution for several reasons. He had no rivals in America, except for Hrdlička (and the latter only to a limited extent), in the study of the primate dentition, and few rivals anywhere. He also ranked in the 1920s and early 1930s as the foremost defender in the U.S. of the theory that man and the great apes shared a recent "brachiating" anthropoid ancestor.[9] As a thorough-going "pithecophile" (his own term[10]) Gregory put forward a view of the earlier stages of hominid emergence that provided, as Hrdlicka's did for the later stages, a strong critique of theories which were in those years pushing the study of paleoanthropology up the "blind alley" of parallel hominid phyla and ancestorless family trees.

In addition, Gregory's evolutionary scenarios are important because they stressed function and adaptation at a time when much of the writing on fossil humans easily bogged down in the discussion of small morphological differences that lacked clear functional importance.[11] While he responded to the uncertain state of evolutionary thinking in his generation by generally avoiding discussions of the genetic mechanisms by which species evolved adaptively, his work helped keep alive a Darwinian approach to charting the path of human evolution. Gregory's continuing influence in paleoanthropology is hard to estimate; yet it does seem clear that this last characteristic of his work was important in maintaining a base upon which the postwar generation could build. Finally, he provided a bridge to the future on at least one specific issue, since he was the first American to make a strong case in defense of the hominid status of the australopithecines.[12]

Gregory's Early Ideas on Evolution and the Formulation of His "Dietary Hypothesis"

While Gregory's first major work relating to human evolution, the classic Studies on the Evolution of the Primates, appeared in 1916, analysis of his early writings on vertebrate evolution provides the background necessary for understanding some basic ideas that informed this and many of his later works. His initial studies relating to mammalian evolution grew out of work he did under Osborn while the latter was preparing The Age of Mammals. Though in some ways the concerns Gregory expresed resembled those of his teacher, right from the start the younger man displayed a careful, critical spirit and a list of theoretical priorities that would push him far from the sorts of phylogenetic conclusions that Osborn would later espouse.

Like Osborn, Gregory saw the establishment of accurate phylogenies as an essential, if not the essential goal of the paleontologist. In spite of the criticisms of contemporary geneticists that such phylogenies were not subject to experimental verification and therefore purely speculative, Gregory asserted, "the time for developing phylogenetic conclusions and for revising comparative anatomy and classification is always now." Also like his mentor, Gregory believed that in analyzing and explaining the morphology of fossil specimens the paleontologist should not shy away from the concept of adaptation. The critics might charge, he conceded, that conclusions about the adaptive function of characters in extinct animals were mere hypotheses, and that appealing to the concept of "progressive adaptation" to explain directional change over time was "both premature and teleological." Nonetheless, he argued, there were well-dated series of fossil mammals which did provide abundant evidence that organic change could be both adaptive to geologically documented environmental conditions and "progressive" in a certain direction over time. Whether such trends had occurred by means of continuous change or small successive gradations might not yet be known; similarly, the types of genetic processes that provided such change might not be understood either. But paleontology could still proceed, pending the determination of such questions, for "when cleared of all implication as to the mode of evolution," progressive adaptation remained as a "historical and verifiable process."[13]

Though Gregory shared some important goals with Osborn, in general his aims were more modest and the principles he invoked less sweeping. Osborn believed that paleontology could reveal "laws" of evolution that genetic analysis could not as yet explain, but which constituted constraints under which genetic processes operated. As we have seen, the most important of these for him were the "laws" of "irreversibility" and "orthogenesis."[14] By the 1920s Gregory had expresed his scepticism about such "laws" clearly, but even in his earlier work he stated a commitment to certain rules of procedure that if followed would temper any speculative tendencies in his own work.

In his first important monograph, The Orders of Mammals,[15] Gregory laid out some groundrules for phylogenetic analysis that revealed his impatience with past paleontological practice. In introducing them, he confessed that his principles might "indeed seem to be obvious councils [sic] of perfection, but so much zoological study has been vitiated by the neglect of them that it has come to be scarcely respectable to draw up a phylogenetic tree."[16] Of the seven that he stated, the most relevant to his future involvement in debates on human origins were the fourth, sixth, and seventh; these warned the researcher to "avoid explaining the little known by the less known, ... make constant reviews to see that no pertinent fact has been omitted, ... and to test again and again his basal assumptions."[17] Obvious as these injunctions might seem, the notorious imperfection of the fossil record made adherence to them difficult in many cases. Phylogenetics, Gregory believed, could not afford the luxury of relying on the existing fossils alone -- it had to employ a process of "triangulation" as well, to search already known species for characters that were suitable "for the backward projection of assumed lines of development to their intersection in undiscovered synthetic types."[18]

Because analogy and extrapolation were dangerous as well as necessary tools, care in their use was essential. This was especially true because Gregory also believed that in "triangulating" one could make use of the characters of existing species along with those of the fossils.[19] Indeed, a hallmark of Gregory's approach to the study of evolution was the way in which he continually tried to interrelate the data of comparative anatomy with that of paleontology.

In addition to providing guidelines on how to look at data, The Orders of Mammals also stated Gregory's preferences about what sorts of data to look for. Morphological description and comparison could be carried to almost endless lengths, and one way he favored for focusing study was to adopt a "historical" rather than a "numerical" approach in choosing characters on which to base phylogenetic conclusions. "The relative age of characters," said Gregory,[20]

> should in all cases be the prime object of research. This historical method (though open to many pitfalls) when judiciously applied seems to be more likely to lead to lasting results than the time honored method of setting down all the resemblances and differences between two animals, without further analysis, and then striking a balance.

Perhaps even more significant for Gregory's later work on primate evolution was a principle of data selection dased on a distinction between types of characters rather than their ages. His initial basis for this distinction was the following hypothesis:[21]

> namely, that the parts which come more directly and simply into relation with special food habits and special environments (such as teeth, claws or hoofs, digestive system, etc.) are more plastic, and frequently of less value as criteria of remote interordinal relationships, than those parts (such as the brain, reproductive organs, foramina of the skull, auditory ossicles, etc.), the relationship of which to the environment is more indirect and complex.

These two categories were, he said, similar to those which had previously been called "adaptive" versus "morphological" in the literature, but Gregory preferred the neologisms "paleotelic" and "caenotelic" for them; the contrast implied in the simpler terms he felt to be misleading, for after all, if "any of these sheltered, persistently surviving paleotelic organs or characters are brought into more direct relations with new conditions, either environmental or somatic, they become just as 'adaptive', or caenotelic as the rest ... a paleotelic character becomes caenotelic by a change of function."[22]

The pair of terms that Gregory had introduced in 1910 was superseded four years later by another, which he would continue to use for the rest of his career -- "habitus" and "heritage." The distinction was basically the same but the focus had shifted from single characters to complexes of characters sharing a common origin and meaning. Applying these concepts to the evolution of fishes, Gregory identified the "habitus" of a species as "the totality of their caenotelic characters, i.e. of all those characters that have been evolved in relation to their latest habits and environment." "Heritage", then, became the sum of "paleotelic characters, i.e. of all those characters which have evolved in relation to earlier habits and environments and which were transmitted in more or less unchanged condition, in spite of later changes of habits and environment."[23]

These new terms, Gregory thought, would be helpful in the task of phylogenetic analysis, for they would remind students that "the habitus of a race tends to conceal its remote phylogenetic relationships; the heritage reveals them." Similar habitus could create analogous structures in species that were not closely related by descent, and major differences in habitus could cause great morphological differences to arise between ancestors and descendants or closely related species that were part of a single adaptive radiation. In both situations closer attention to "heritage" characters would make accurate phylogenetic assessments more likely. Still, as with his earlier terms, Gregory was quick to point out that the line between the two types of characters was not immutable: "heritage" characters could be assimilated into "habitus" with changes in function and environment, and "habitus" into "heritage" with the absence of such changes over long periods of time.[24]

A thorough analysis of the meaning and implications of the habitus-heritage distinction would be beyond the scope of the present essay. It appears, however, to be similar to the contrast between "primitive" and "derived" characters employed more recently in phylogenetics, but with one crucial difference. The habitus-heritage distinction could be used only when it was combined with hypotheses about adaptations and functions for the characters involved. This characteristic of the concept is critical for an understanding of Gregory's approach to primate evolution and evolution in general. Though his data came largely from description and comparison of "dead" morphological detail, he continually tried to place it in a context which also contained hypotheses about how the living organism had functioned in relation to its environment. In a way Gregory could be said to have been directly following the lead of his mentor in stressing the critical importance of adaptive patterns on evolution; Osborn, however, tended to be somewhat arbitrary in his invocations of the organism's responsiveness to environmental change. Certain orthogenetic trends in his view were mandatory, especially among the mammals.[25] The way he conceived of the "habitus-heritage" concept gave Gregory a means of insisting on a greater plasticity of animal life and especially its potential for transformation of form in response to environment.

In addition to these general considerations of theory, there was a specific aspect of adaptation in vertebrates which concerned Gregory early in his career and later came to occupy a major part of his discussions on higher primate evolution, namely, the analysis of locomotor adaptation. Here again ideas of Osborn provided the jumping off point. In his work on herbivorous mammals Osborn had identified two styles of locomotion which seemed to be polar opposites, and which required completely different adaptive complexes to effect. One, displayed in its truest form by the elephant, Osborn had called "graviportal," while the other, characteristic especially of the horse family, he named "cursorial."[26] In order to develop this concept more rigorously, in 1912 Gregory published a biomechanical analysis of these two styles of locomotion; his method involved comparing the limb structure of each type of animal to a system of compound levers. Taking the ratios between the lengths of various pairs of limb segments in recent mammals from each type as his base, Gregory then proceeded to examine the limb proportions of various fossil mammals in order to develop hypotheses about their locomotor adaptations, and by these means to assess their phylogenetic position more exactly.[27]

As Gregory was well aware, neither the typology of styles of locomotion nor the concept of limbs as levers was original with him. Still, this work is important, since it foreshadowed his interest in locomotor adaptation in the higher primates -- particularly in the concept that humans had a "heritage" that involved "brachiation," as well as the idea that recent "habitus" differences attendant upon a shift in locomotor style were mainly responsible for many of the skeletal differences between humans and pongids. While Gregory did not devote a great deal of his own research effort to compiling data on this question,[28] his sensitivity to it put him in a position to derive the maximum phylogenetic mileage from such data as it appeared. An important example is the use he would make of the pioneering biomechanical studies on the evolution of the human foot conducted by his colleague at Columbia, the anatomist Dudley J. Morton.[29]

Though Gregory's early work on "habitus" and "heritage", and on locomotion, displayed an interest in the study of animal function that was to be life long, it is important to point out that he pursued this interest largely through the traditional method of paleontology and comparative anatomy -- qualitative morphological description and comparison. Yet though the method was traditional, his stress on the principle of adaptation, his sensitivity to the overall weight of the evidence before him and his flexibility in interpreting that evidence combined to give his writings a kind of freshness that the works of his contemporaries often lacked. The durability of Gregory's approach can be seen nowhere better than in his first monograph relating to the problem of human evolution -- Studies on the Evolution of the

Primates.[30]

Studies on the Evolution of the Primates looms as a landmark in American physical anthropology for several reasons. Most significant, it provided the first exposition of Gregory's ideas on the "Dryopithecus pattern" in the lower molars of fossil and recent hominoids, and thus laid the groundwork for Gregory's contention that the ancestors of both modern humans and pongids had been products of adaptive radiation within the dryopithecine group. To buttress conclusions based on the study of the teeth he also produced the first major defense in the U.S. of the theory advanced by Sir Arthur Keith regarding human descent from a "brachiating" ape ancestor. Finally, Gregory placed his phylogenetic conclusions within an adaptive framework, formulating a theory of human emergence based principally on a change of "food habits." This theory attempted not only to account for the differences in dentition, skull form and locomotor apparatus between modern apes and humans, but also to explain differences between Homo sapiens and then known fossil hominids as consequences of increasingly effective adaptation to the human ecological niche. This forceful attempt at synthesis made Studies a model for later work on the primate dentition and a foundation for all of Gregory's later forays into anthropological controversy.

The most original part of Studies was the analysis of higher primate dental evolution, and especially that of the molar teeth. Here, as elsewhere in the earlier part of his career, he was building upon, and refining, ideas that Osborn had tried to develop previously. In this case Gregory was undertaking to develop further the so-called "Cope-Osborn theory of trituberculy," which had hypothesized the existence of homologies between the cusps of the molar teeth of early insectivorous mammals and those of later mammalian forms.[31] Osborn himself had traced the path of evolution in the molar teeth most extensively in the ungulate mammals,[32] but Gregory believed the former's system of nomenclature, and the general trend discerned, were supported by the primate fossil record as well -- the trend being that "the primitive tuberculo-sectorial lower molar," which had been "provided with small cutting blades and sharp points for an insectivorous diet," had been "transformed into a bluntly cusped, crushing molar adapted for omnivorous or for herbivorous diet."[33]

Within the resulting relatively flat, approximately "quadritubercular" form common to higher primates (apes, monkeys and humans), there were, Gregory thought, major distinctions to be made, and within such distinctions he attempted to discover homologies that would reveal phylogenetic relationships. His critical discovery was that of the so-called "Dryopithecus" or "Y5" pattern in hominoid lower molars. The former designation stemmed from Gregory's contention that this pattern had first emerged among various members of the dryopithecines, a group of fossil hominoids occurring in Miocene and Pliocene deposits of the Old World. The latter name referred to the actual crown pattern of the "typical Dryopithecus" lower molar -- i.e., it possessed five cusps, with three on the half closer to the cheek, and two on the half nearer to the tongue, along with a system of furrows separating the cusps that appeared to form an inverted "Y" when viewed from above (See Figure 3).[34]

To Gregory the evolutionary significance of the "Dryopithecus" pattern was clear -- it was directly ancestral to the lower molar patterns of both humans and the large-bodied apes -- gorillas, chimpanzees and orangs.[35] In recent specimens from all three pongids lower molars could be found which deviated from the ancestral pattern only in minor specializations; in the fossil hominids "Homo Heidelbergensis" and "Homo neanderthalensis," as well as in modern "primitive races," Gregory also was able to find teeth which agreed fundamentally in both number and disposition of cusps with dryopithecine specimens.

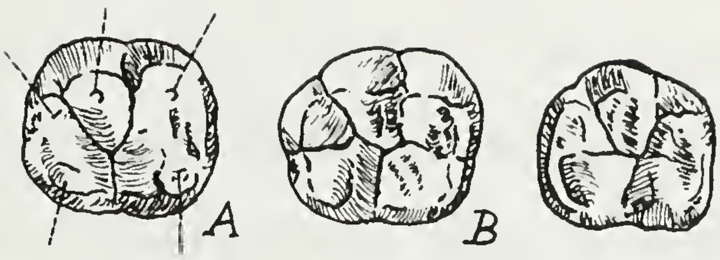

Figure 3. The "*Dryopithecus* pattern" in hominoid first lower molars. B is a "primitive" specimen from a recent human skull, compared with two Siwalik dryopithecines: *Sivapithecus indicus* (A), and *Dryopithecus chinjensis* (C). The five principal molar cusps are indicated by the dotted lines on tooth A. After Gregory, "Studies on the Evolution of the Primates."

The fact that the "typical" lower molar in "advanced" humans had only four major cusps and a "+" shaped pattern of furrows was not a major obstacle, since this "+4" pattern on a less rectangular "subcircular" crown (another "advanced" human character) could easily have been produced by progressive reduction of one of the five major cusps of the "Dryopithecus" molar, the hypoconulid.[36] That such a reduction had occurred also seemed quite likely in the context of Gregory's belief that a change in food habits was a critical factor in human emergence from the dryopithecine stock that had also produced humankind's more "conservative" relatives.

To establish that such a change had occurred Gregory looked beyond the molar teeth to other aspects of the dentition and skull form among hominoids. Here the fossil evidence was highly fragmentary, but he called the data of comparative anatomy into service to fill in the gaps. The key to the problem thus lay in an analysis of the main differences between humans and the apes he believed to be their closest living relatives, the chimpanzee and the gorilla (at this stage in his work Gregory seemed to believe that the latter might actually be the nearest of kin, as witnessed by his remark that "the young female gorilla, except in the dentition, more distinctly approaches the human type than any other anthropoid"[37]). These two forms could be used as stand-ins for the dryopithecine common ancestor, he thought, because they had "retained, with only minor changes, the ancestral habits and habitus in brain, dentition, skull and limbs."[38]

In Gregory's view these "ancestral habits" and the "habitus" that had accompanied them involved principally adaptations to a largely frugivorous diet in a forested habitat.[39] Frugivorous, large bodied anthropoids, he contended, required "powerful jaws and teeth," and particularly "powerful canine tusks and more or less procumbent incisors for tearing open the tough rinds of large fruits and for fighting."[40] In tandem with this robust and powerful dentition went the muzzle-like appearance of the face and the massive lower jaw of these apes, along with the "outgrowth of bony ridges between, above and outside the orbits," since all of these structures were necessary to provide support for the dental apparatus. Bony crests along the top and rear of the skull, which appeared most prominently in male gorillas, also were present, he argued, to provide areas of insertion for the massive jaw and neck muscles required to work the dentition and support the head.[41]

When Gregory contrasted each element in this complex of characters with its counterpart in humans, he concluded that the human characters no less than those in the pongid species constituted a "functionally correlated series." In addition, he argued that these "distinctively human features" were "relatively late specializations" which had appeared and developed in association with each other. Chief among the characters that related most directly to the dentition he counted the following:[42]

> (1) shortening of the muzzle and symphysis.
> (2) Retraction of all the anterior teeth, the incisors becoming more erect, the canines decreasing in size and the "edge to edge bite" becoming further emphasized.
> (3) Reduction in size of the front lower premolar and the completion of its bicuspid character.
> (4) Development of a chin (a late feature).
> (5) Increasing convergence of the opposite tooth rows and widening of the intercondylar diameter of the mandible.
> (6) Rounding of the molar crowns, progressive obliteration of the anthropoid, or Dryopithecus pattern of the molars and in some cases progressive loss of the hypoconulid on the second and third molars. Progressive reduction of the third lower molar from a more quadrilateral to the more tritubercular pattern.
> (7) A change in the predominant movement of the mandible from a more ruminant-like, obliquely transverse movement, to movements in all directions and of a partly rotary character. (Especially correlated with the reduction of the canines.) ...
> (9) a final shifting and readjustment of the whole lower dental arch in such a manner that the upper incisors finally overhung the lower incisors, and that each lower molar, which formerly articulated with two upper molars, came to articulate chiefly with only one upper molar.

As point number eight in the above list, Gregory had pointed out that some of humankind's distinctive cranial characters had been partly influenced as well by a set of correlated changes associated with "the assumption of the upright posture, the enormous increase in the braincase and the consequent balancing of the head upon the neck."[43] This created no uncertainty in his phylogenetic analysis, for he saw no problem in deriving bipedal hominids from "semi-erect" quadrupedal apes. Indeed, asserted Gregory, African pongids' "peculiar method of taking great strides with the forearms in a semi-erect posture, again forms a necessary prelude to fully erect bipedal progression."[44]

The question of posture and its evolution raised important questions in primate comparative anatomy and Gregory made his interpretation of the existing evidence clear. A critical point in that interpretation was his full agreement with Keith that "brachiation" or "swinging from branch to branch with the arms" had been an important preadaptation, a "necessary introduction to the upright posture of man."[45] This form of locomotion, he believed, had "trained the arms in the all important power of supination and improved the brain, eyes and all the balancing mechanism." The upright sitting and squatting postures seen in the brachiating apes had also been essential as they had "conditioned the loss of the tail and the further development of all those powers of the backbone, thorax and pelvis which give to the anthropoid skeleton a distinctly subhuman look. Moreover, the habit of sitting upright tended greatly to encourage the use of the hands."[46]

If the upper body of the African apes seemed preadapted for a later transformation in a hominid direction, so in his estimation did the foot of these creatures, especially that of the gorilla. Gregory listed the distinctive characters in the feet of lemuroids, Old World monkeys and gorillas, and the ways in which the latter differed from humans, and concluded that by "relatively slight morphological changes a gorilloid type of foot could easily be made over for service on the ground" according to the human pattern.[47] He was willing to concede the possibility that the gorilla, by a parallel adoption of terrestrial habits, was "acquiring at a late date characters which the Hominidae acquired far more effectly at a far earlier period, perhaps in the Upper Miocene." But the multiple resemblances and the fact that the human foot would probably have had to pass through a structural stage very similar to that evidenced by the gorilla seemed persuasive evidence in favor of recent common ancestry.[48]

The abundant use that Gregory made of the gorilla for his anatomical arguments and his feeling that the female gorilla approached human proportions more closely than the other pongids could eaily have left the impression that he was advocating a gorilloid ancestry for humans. In his actual phylogenetic hypotheses he did not go in this direction, however, and disavowed any claim that the "Hominidae were derived from any still existing genus of anthropoids."[49] Instead, he asserted, both the data on fossil teeth and that from comparative anatomy converged on the hypothesis that "the ancestral chimpanzee - gorilla - man - stock" appeared to be "represented by the Upper Miocene genera Sivapithecus and Dryopithecus, the former more closely allied to, or directly ancestral to, the Hominidae, the latter to the chimpanzee and gorilla."[50]

The language Gregory employed seemed to imply that the divergence of hominids and their closest ape relatives from the common ancestral stock might have occurred prior to the appearance of these dryopithecine genera, but lest anyone use this opening to force the time of hominid emergence too far into the past he added that "at present I know of no good evidence for believing that the separation of the Hominidae from the Simiidae took place any earlier than the Miocene, and probably the Upper Miocene." Anticipating the objection that this date left too little time for a generalized "semi-arboreal, semi-erect and semi-quadrupedal" dryopithecine to have evolved into humans, Gregory argued as follows:[51]

> the change in structure during this vast interval (two or more million years) is much greater in the Hominidae than in the conservative anthropoids, but it is not unlikely that during a profound change of life habits evolution sometimes proceeds more rapidly than in the more familiar cases where uninterrupted progressive adaptations proceed in a given direction.

In order to explain how the "functionally correlated series" of changes that he had identified could actually have occurred, Gregory had to go beyond citing a "profound change in life habits" and actually produce a scenario that correlated functional with structural change. The scenario had to be a speculative one, given the character of the paleontological and archeological data, but he was certain that food habits had played a central role in the transformation. The key transition, he felt, was from a largely "frugivorous" diet common among anthropoids to an "omnivorous" diet, and one in which meat became an increasingly important item. Gregory noted that gorillas and chimpanzees had digestive tracts very similar to humans', and that zoo specimens from both species had been known to eat small quantities of meat. Also, naturalists had apparently observed them in the wild to "greedily devour young birds, as well as eggs, vermin and small rodents." In addition all the existing anthropoids seemed to show a significant capacity for intelligent use of the hand. It was but a short step from these observations to the hypothesis that[52]

> at a time when tough-rined [sic] tubers and fruits were still the main element of the diet the nascent Hominidae may have sought out the lairs and nesting places of many animals for the purpose of stealing the young and thus they may have learned to fight with and kill the enraged parents ... and possibly they killed both by biting, as in carnivores, and by strangling, or in the case of a small animal, by dashing it violently down.

The next step in the evolution of the hunting habit would have involved the beginnings of tool use, and Gregory, like Osborn and MacCurdy as well, found it plausible to imagine that once early hominids (in the course of their dispersal from a "south central Asiatic centre") had entered regions where flints could be obtained easily, they had learned how "Eolith" flints "could be used to smash open the head of a small strangled animal, to crack open tough vegetables, or to mash substances into an edible condition." The use of these stones in the chase would have followed, perhaps after a period in which they were primarily used to defend against "intruders" which had surprised the hominids while they were using their primitive tools for food preparation.[53]

While the hominids were in the midst of this transition toward a diet that included substantial quantities of meat, as well as toward the use of tools for procuring and preparing food, Gregory hypothesized that major changes in the dentition had been taking place as well. First, the front of the dental arch had begun to "retract" -- the incisors becoming smaller and more vertical, thus producing an "'edge to edge bite' of the upper and lower incisors ... well fitted for pulling and tearing meat from bones." The canines, no longer so crucial for fighting (with the advent of weapons) or for piercing tough vegetable food, also had become smaller and more vertical; no longer protruding above the tooth row, they had thus made a more rotary motion of the jaw effective in mastication, and had been able to assist the incisors in cutting and tearing.[54] The fact that the front teeth of these early hominids would have been small and weak compared to those of other carnivores of similar body size did not seem to be a problem for two reasons -- first, he noted, was the mechanical fact that "with a given muscular power small teeth are more easily forced into meat than large teeth;" second, with the beginnings of culture already in progress bipedal hominids would have been using "rough flints" both "to tear the flesh and to puncture the bones" of their prey and to fight, while quadrupedal carnivores had to do both with their teeth.[55]

Continued improvements in tool technology, and the discovery and elaboration of other cultural practices like the use of fire would have had a further influence on reducing the dentition and other cranial structures. This process, Gregory thought, had probably continued throughout the course of the Pleistocene; in fact, like Hrdlicka, he felt that it had continued even after the appearance of anatomically modern Homo sapiens, noting that its final stages had "probably had to await the development of vessels for holding hot water, perhaps in Neolithic times."[56]

Gregory was aware that his language in these passages, connecting as it did changes in "habit" with morphological change, was open to the charge that it appealed to "Larmarckian" processes. However, though he recognized the strictures of the "experimentalist" against this form of inheritance, Gregory maintained that "nobody with a practical knowledge of the mechanical interaction of the upper and lower teeth of mammals, or of the progressive changes in the evolution of shearing and grinding teeth, can doubt that the dentition has evolved pari passu with changes in food habits." While the phrasing of other passages had a Lamarckian "ring," Gregory's use of the term "pari passu" could support a selectionist interpretation as well. In fact, in concluding he indicated that he was not opting for either hypothesis concerning the mechanism of hereditary change, but was only charting the path that change had taken:[57]

> whether, as commonly supposed, the food habits changed before the dentition, or vice versa, the evidence appears to show that the Hominidae passed through the following stages of evolution:
> 1) a chiefly frugivorous stage, with large canines and parallel rows of cheek teeth (cf. Sivapithecus).
> 2) a predatory, omnivorous stage, with reduced canines and convergent tooth rows (cf. Homo heidelbergensis).
> 3) a stage in which the food is softened by cooking and the dentition is more or less reduced in size and retrograde in character, as in modernized types of Homo sapiens.

Gregory's scenario of hominid evolution had great explanatory power. Though it did not clear up some difficult questions such as the initial causes of the bipedal adaptation or the causes and timing of brain expansion, it nevertheless provided a parsimonious, not to say elegant, intellectual framework that joined together most of the data that then existed on the comparative anatomy and paleontology of higher primates. In fact, Gregory's "dietary hypothesis" as it was developed in 1916 was in its main lines clearer and more persuasive then competing scenarios that would be put forward by such diverse figures as Osborn, MacCurdy and Hooton. Indeed, it could be said, as far as its discussion of fossil hominids was concerned, to have surpassed most of

Gregory's own later writings in these qualities.

In large measure this clarity and persuasiveness proceeded from the simple fact that in his discussion of fossil hominids he presented a basically unilinear interpretation of the evidence that dovetailed with the unilinear impetus of his general evolutionary scenario. I say "basically" because the evidence with which he was working was fragmentary and somewhat misleading. The first problem lay with the most "primitive" human fossil then known, "Pithecanthropus erectus." Eugene Dubois had found some fossil molars in Java which he had associated with the hominid skull cap of "Pithecanthropus;" after examining the casts, however, Gregory found them to be "remarkable" for their size, their widely divergent roots, and for the marked similarity of the "contour of the crown" to orang molars. The last feature seemed so much closer to pongid than to "human types" that Gregory, rather than question Dubois' attribution of the teeth to Java man, put the whole fossil into a sort of paleontological limbo, merely citing the "possibility" that "Pithecanthropus" might have been "related both to _Homo_ and to _Sivapithecus_."[58]

Though he could make little use of "Pithecanthropus," there was one undoubtedly ancient specimen of humankind that Gregory believed to provide strong support for his evolutionary scenario -- the Mauer jaw, then known as "Homo heidelbergensis." In his view the extremely well-preserved teeth of the Mauer specimen bore indisputable marks of the human adaptive pattern. Thus, the canines were small and did not protrude above the rest of the tooth row, the dental arch converged toward the front in the human manner, and the "vertically placed" incisors had the sharply worn tips indicative of the human "edge to edge bite." At the same time, he contended, the teeth showed "primitive" characters that were "frequently lost in the higher types" of the modern human jaw; for example, the molars retained the fifth cusp, the "hypoconulid," more of the Dryopithecus pattern of furrows, and the dental arch was not quite so "convex." In addition the mandible had primitive characters, such as "the lack of a chin," which seemed to him to "recall the ancestral anthropoid characters."[59]

Despite these "primitive" characters, Gregory argued, the dentition of "Homo heidelbergensis" was "typically human" overall; that fact, and the fossil's great age, seemed to show that the "transition from anthropoid to human characters in the dentition took place at an epoch far anterior to the Mid-Pleistocene." The ensemble of characters present in the jaw also indicated to him that Heidelberg man was a key figure in the transition to Homo sapiens, and not an "aberrant side line" in hominid evolution. Since it differed from Homo sapiens, and from Neanderthal man as well "only in its more primitive characters" and greater age, he concluded that he could recognize "no character in this species that would definitely exclude it from ancestry to Homo sapiens." Accordingly, he was willing to speculate that Heidelberg man was apparently "directly ancestral to all the later Hominidae."[60]

Gregory anticipated one paleontological objection to his conclusions about the Mauer jaw, and responded to it with a significant corollary to his "dietary hypothesis." Noting that the "gigantic size" of the jaw might be looked upon as a specialization excluding that creature from direct human ancestry, on the grounds that "in many other phyla of mammals the gigantic members are supposed not to be ancestral to the smaller existing races," he countered with the following argument:[61]

> however it might have been in other phyla, a large stature, or more precisely a massive head and thorax, may well be expected in the ancestral Hominidae. When the ape-man definitely abandoned the forests and intruded themselves into the gigantic and well-armed fauna of the plains we may be sure that there was no place for undersized gibbon-like beings of pacific habits, but all the conditions at first favored the evolution of powerful and aggressive hunters and fighters, killing with the crudest weapons and tearing off the raw meat with their powerful jaws.

In tandem with this early version of what has come to be called the "killer ape" theory, Gregory also speculated on the basis of the Mauer jaw's "lower type" in comparison to the Neanderthals that Heidelberg man's "intelligence was also of a lower order, the face extremely heavy, and the forehead retreating." The existing cultural evidence, namely the "total absence of palaeoliths or other artifacts (aside from the highly questionable eoliths)," also tended to support his idea that "the earlier races were much less intelligent than the Neanderthals, who knew how to make a number of kinds of stone implements."[62] In sum, the Mauer jaw's giant size was just what one would have expected of a bipedal carnivore with a much smaller complement of intellectual and cultural resources than his successors would enjoy.

Once developed, this corollary -- that primitive characters such as robustness in the skull and dentition reminiscent of anthropoids could not exclude an earlier hominid fossil from ancestry to later forms of humanity -- proved useful in evaluating the Neanderthals as well as Heidelberg man. Gregory was aware of the recent trend toward excluding the Neanderthals from human ancestry, but he did not choose to follow the path laid down by Boule and Keith largely because of this principle. The Neanderthal dentition had one alleged characteristic that involved a slightly different problem, however -- the so-called "taurodonty" of the Neanderthal molars.

The "taurodont" molars, (the term was Keith's) first described by the German scientist Hermann Adloff, were marked by a large pulp cavity and by roots that were "not sharply constricted from the crowns" but instead formed "with the crowns a stout column, tapering gently toward the bottom and extending deeply into the jaw." According to Adloff, "taurodonty" had been a unique Neanderthal specialization, while Keith had claimed to identify it also in an incipient stage in Heidelberg man. Keith had also alleged that "taurodonty" appeared to have been a special adaptation for a "rough vegetable diet;" as such, it provided the latter with another indication that the Neanderthals were a hominid branch separate from the one leading to modern humankind.[63]

While Gregory was willing to accept the existence of "taurodonty" as a common character among the Neanderthals, he could not accept Keith's theories about its meaning. First, that "taurodonty" was a vegetarian specialization seemed unlikely to him, for Neanderthal sites like Krapina clearly revealed Neanderthal man's "prowess as a hunter." If this were so, he said, one could "hardly deny that animal food formed a large part of his diet." In fact, though the tools of the Neanderthals had helped them to procure and butcher animal food, perhaps the "taurodont" molar was correlated with heavy meat eating itself. After all, he suggested, these "very stout deeply implanted molars, with their rough surfaces, would be well adapted for chewing ... meat and crushing small bones."[64] Gregory even went beyond this, to question whether "taurodonty" was truly a Neanderthal specialization. His own observations had convinced him that there was significant variability in the degree of taurodontism among Neanderthals and Homo sapiens as well. The range was so wide that one could find molars among modern primitive peoples that were "more like those of the Krapina men than they are like those of typical white men." To him this indicated the strong possibility that there had been "a loss of 'taurodontism' in Homo sapiens correlated with the reduction in size of the jaws and with the use of cooked food." This explanation, which was similar to Hrdlicka's account of the trend toward the reduction of the "shovel-shaped" incisor,[65] fit in much better of course with Gregory's general formulation of the evolution of food habits among the hominids.

In assessing the general phylogenetic position of the Neanderthals Gregory felt free to ignore supposed "specializations" and to reason about them in a way similar to his argument concerning Heidelberg man. He was unilinear picture of human evolution, in his final conclusions on the Neanderthals he made concessions to the multilinear theorists. Thus, in the phylogenetic tree he drew Heidelberg man remained on the main line, but "Homo neanderthalensis" went out on a side branch; he also raised the possibility in the text that "the Cro-Magnons and other high types may well have come into Europe from Asia and the European Neanderthals may simply have crossed with the invading race." Surrender to the "early-sapiens" idea this was not, however, but rather an early form of the so-called "pre-Neanderthal" theory that was later filled out by F. Clark Howell.[68] A "Neanderthaloid" stage, Gregory asserted, was a necessary one in human evolution; the Cro-Magnons may have come in from the east,[69] but

> the ancestors of the Cro-Magnons in Asia must have at some time have [sic] passed through a Neanderthaloid stage of evolution and perhaps it was some of the older strains of these pro-Neanderthaloids and not the Mousterian population, which may have given rise at different times and in widely separated regions to the composite group called Homo sapiens.

Gregory's dietary hypothesis, then, not only provided a plausible account of the fragmentary data on hominoid relationships, but it also acted as a corrective on the "splitting" tendency common in studies of fossil hominids at the time, studies in which, as Gregory noted, thus willing to grant Boule's contention that " <u>Homo neanderthalensis</u> abounds in low characters not found in such an assemblage in any existing specimens of man." But as Hrdlička was arguing at this time as well,[66] Gregory asserted that "no one and no group of these characters" appeared "to exclude this species either from derivation from upper Miocene anthropoids or from ancestry to <u>Homo sapiens</u>." The great robustness of the skull, jaws and dentition of the Neanderthals was in full agreement with his general theory that "the ancestral Hominidae" had been "ferocious and predatory terrestrial anthropoids." That the Neanderthals surpassed more recent humans in the "great size and depth of the face" was also no surprise, since the latter were "all more or less retrogressive in the face, dentition and jaws, and highly progressive in the brain case." Thus the large number of the morphological distinctions Boule had made, such as "all the wide differences in crancial indices," between the Neaderthals and later populations failed to exclude the former from ancestry to the latter.[67]

While he did not discuss it in detail, the cultural and stratigraphical evidence that had so impressed writers like Osborn and MacCurdy in regard to the Neanderthal question must also have had some impact on Gregory. Though the whole tone of his discussion had been toward a

Homo sapiens had been "left without known ancestors in the latest phylogenies."[70] There was one new "fossil," though, which could only with great difficulty have been fitted into Gregory's alternative scenario -- and that of course was Piltdown man. Even with teeth faked to approximate human patterns of wear, the Piltdown mandible diverged greatly from Gregory's conception of the primitive hominid dental apparatus. Indeed, it could be said that the Piltdown and Mauer jaws could hardly inhabit the same evolutionary scenario.

Fortunately, Gregory had a partial solution of his dilemma at hand, for like Hrdlicka, he was an early and strong supporter of Gerrit S. Miller's critique of the Piltdown jaw. Miller, it will be remembered, had compared casts and photographs of the jaw with recent chimpanzee jaws, and Gregory supported the former's conclusion that it was "generically identical" with those jaws. On the contested issue of how the ape-like jaw would have articuated with the human-looking skull, Gregory also seconded Miller's conclusion that, even though the relevant connecting parts were missing, the likelihood that the two could have articulated with each other was very small. There were thus, in Gregory's opinion as well as Miller's, two hominoid fossils at Piltdown -- "Homo dawsoni" and "Pan vetus."[71]

Once the "anomalous composite" had been "resolved into its diverse elements," the way was open for the Mauer jaw to retain its importance as proof of an early transition in the dentition from anthropoid to human patterns.[72] A serious problem still remained, however -- the placement of the Piltdown skull in relation to other hominid fossils. In discussing this problem Gregory noted how, in his initial studies of casts of the Piltdown specimens in 1914, he had been "impressed" by the fact the skull had "offered no salient distinctions from Homo sapiens, the most remarkable feature being their great thickness." Unlike Hrdlicka, however, who used this fact to justify his doubts about the skull's geological antiquity, Gregory was still willing to work on the assumption that Piltdown man was ancient, even though he saw that the "precise age" of the specimen had never been "positively settled." If the Piltdown skull was old, and had no jaw associated with it, he wondered, why not fit it to the Heidelberg jaw? Both seemed to have close affinities to Homo sapiens except for certain primitive characters (more numerous in the Mauer jaw than in the Piltdown skull, of course). If there were an association between these two fossils, he asserted hopefully, it would "go far toward clearing up the origin of Homo sapiens."[73]

Gregory would only venture that such an association was "suspected," but the fact that he was even willing to entertain the possibility showed the abundant potential for causing confusion that Piltdown possessed. For the smooth-browed, and by implication relatively small-faced, creature represented by the Piltdown skull not only contrasted with the well-known skull form of the Neanderthals, but contradicted Gregory's own hypothesis about what the face and skull of Heidelberg man had probably looked like.[74] The ferocious predator with the robust and muscular head and thorax, and the conception of human evolution that had given rise to this picture, seemed to have been forgotten in the midst of Gregory's solicitude lest "Homo dawsoni" lose its importance in paleoanthropology. It is interesting to note that he failed to hit upon an association between the Heidelberg jaw and the only other ancient hominid fossil then known, the "Pithecanthropus" skull, for such an association would have met the requirements of his theory far better than the one he was suggesting. It seems as though, in common with Osborn and others, the "primitiveness" of the Java specimen made it difficult for him to see the latter as even being close in geological time to the appearance of really "human-like" forms like the Neanderthals or even Heidelberg man.[75]

These speculations about Piltdown man should not be emphasized too much, for they were offered hesitantly and in so general a form that they detracted little from the force of Gregory's larger argument. In _Studies on the Evolution of the Primates_ he had clearly succeeded in producing a cogent statement of his "dietary hypothesis," and had staked out his major lines of defense against opposing theories.[76] Between 1916 and 1920 he did not publish any significant additions to his theories on the evolution of the dental apparatus, but his research on the subject, and on the evolution of the primates in particular, continued. The first results of this work to appear were a preliminary revision of the phylogenetic relationships of certain Eocene lemuroids, and the initial installments in what was to be a long series of articles and monographs on key stages in the evolution of the human face.[77] 1920, however, saw major landmarks in Gregory's writings on primate evolution -- his classic monograph on the Eocene primate _Notharctus,_ and more important for the study of the higher primates, a serially published monograph in the _Journal of Dental Research_ on the _Origin and Evolution of the Human Dentition._[78]

The Evolutionary Scenario Refined, 1920--1926

In Origin and Evolution of the Human Dentition Gregory tried to extend the coverage of and to refine the argument that he had put forward in 1916. Though the resulting monograph was much longer (it ran about 500 pages in book form) than Studies on the Evolution of the Primates, the basic interpretation almost duplicates the one in the earlier work, even to the point of reproducing long passages from that work at key junctures in his discussion.[79] To fill out his basic framework, however, Gregory added large amounts of detail on issues like homologies in the dentition among all major primate groups (parts 3 and 4), and on the evolution of the teeth from the earliest vertebrates to the Eocene primates (parts 1 and 2). He also tried to strengthen his account of the later stages in the evolution of the hominids by adding more detail on the Neanderthal dentition and by compiling data on dental variation among modern human groups (part 5).

Though the general interpretation was the same, Gregory was not merely piling up data for its own sake. The material on earlier mammalian evolution served to demonstrate more fully that his revised variant of the "tritubercular" theory was a convincing one; that on the lemuroids, New World monkeys, and Old World monkeys served to shore up the hypothesis that humans must look to the African great apes to find their closest living relatives. The data on modern humans, by identifying in more detail the series of "low" or "primitive" characters that could be contrasted with the "high" or "specialized" conditions encountered among the most civilized groups, provided confirmation of the trend toward reduction of the dentition that Gregory had sketched out earlier. The extremely broad sweep of the monograph was itself intended to prove a point -- namely, that competing theories of hominid evolution, whether they dealt with the dentition or with other parts of the skeleton, often read too much significance into small similarities and differences among a limited sampling of species, and as a result produced phylogenies which thorough analysis of all the main lines of primate evolution could not support.

While all these features of the _Origin and Evolution of the Human Dentition_ are important, for present purposes the focus must be on the same issues that dominated _Studies on the Evolution of the Primates_ -- the interpretation of the comparative anatomy of humans and pongids, as well as that of fossils of both groups. On both these topics the conclusions were largely the same as before. Indeed, while the descriptions of the dentitions were more painstaking, the actual fossil material described remained largely the same, for no accounts of major discoveries -- with the dubious exception of the Piltdown II skull and tooth fragments -- had been published in the interim.[80] There were, however, some refinements in the details of Gregory's interpretation of various fossils, refinements that brought his evolutionary scenario more into line with what was then majority opinion on human emergence.

The most obvious case concerned the most "primitive" hominid -- "Pithecanthropus." In 1916 he had not known quite how to interpret it, especially as he had favored dissociating the skull top from the teeth. Now he was willing to put them back together, along with the femur that Dubois had also attributed to "Pithecanthropus." The association created a new problem, for he had revised his judgement on the morphology of one of the teeth, an upper molar. This tooth, he said, had a "pronounced reduction in the size of the posterior moiety of the crown," of a sort that was normally the product of "degenerative processes in the dentition of the most advanced and presumably late races of man." Rather than question that interpretation of molar crown form, Gregory (somewhat rashly) decided that this was a case of "premature specialization," and one which tended to "remove that genus [Pithecanthropus] from the line of ascent leading to later human races."[81] Even though he had thus taken Java man out of his evolutionary picture entirely, he still felt called upon to account for the primitive "gibbon-like skull top" that it supposedly possessed. In order to do so he went to the same idea that we have already seen in others who thought about this issue -- i.e. that "Pithecanthropus" represented an earlier structural stage than its date apparently would indicate. Thus, he conjectured that "perhaps" the ape-man had been a morphologically conservative creature "which had early been driven away from the primitive dispersal center ... by the pressure of higher races," and had lived on, relatively unchanged, in Java.[82]

The use of the term "primitive dispersal center" raises the obvious question of where such a center might have been located. In 1916 Gregory had indicated the possibility of "south central Asia," citing Matthew's "Climate and Evolution" as suppport;[83] in 1920 he shifted a little to "west central Asia," but was still clearly in Matthew's camp. This continued support came through in the context of his response to the question of why no well-dated fossils of late Tertiary hominids had been discovered so far. His answer reflected what one might call the "Matthew-Barrell" version of Osborn's "Holarctic" theory of mammalian distribution -- i.e. that "during the late Tertiary the Hominidae had not yet extensively invaded the plains, and that in some restricted and more or less isolated Palaearctic region they were in the course of differentiation from ground living apes inhabiting the border regions between forests and plains."[84]

Though he now considered it a side branch in hominid evolution, "Pithecanthropus," by being worked into this variant of the central Asian theory, could be made to stand in for the missing "late Tertiary" representatives of the main hominid line. Thus, in his "Final Summary" of "Twenty-Six Structural Stages in the Ascent of Man and the Evolution of the Human Dentition," Gregory sketched his twenty-fourth stage as follows: "Human pre-cursors. Pliocene. Pithecanthropus of the Pliocene or Lower Pleistocene (?) of Java possibly a representative."[85] Almost totally on the basis of the skull cap, and this probably for the reason that the brain was supposedly small and primitive, Java man finally got a place, if not one that conformed to its true geological age. In this position it could provide some sort of bridge between the dryopithecines and the next stage, the "Heidelberg race," which he continued to defend as the likely ancestor of all later human types.[86]

While Gregory maintained his interpretation of "Homo heidelbergensis" practically unaltered, he did add some significant points to his treatment of the other major group of anatomically "primitive" human fossils -- the Neanderthals. The amount of detail he included was greater as well, especially in the extensive description of one of the best preserved of the Neanderthal specimens -- the skeleton of the so-called "Mousterian youth" uncovered at Le Moustier in 1908.[87] Analysis of the teeth of this fossil, when compared with those of the Mauer jaw and other Neanderthaloid specimens from Krapina, did not lead to major new evaluations; rather, it seemed to confirm his earlier view that in the Neanderthals as a whole one found "very primitive features in the crown pattern associated with at least a moderate degree of taurodontism."[88] He could thus theorize more strongly than before that this "moderate" taurodontism might be a "primitive character for the Hominidae." Thus, it was not one which could be used to exclude the Neanderthals from the main line of human evolution, for if, as he thought, "degeneration or loss of characters has played a large part in human evolution, the loss or reduction of taurodontism" was "easily conceivable."[89]

As with taurodontism, so he proceeded with the other supposed "Neanderthal specializations," which in this work he highlighted more than he had in 1916. Thus, he reported as facts Boule's picture of the Neanderthal head and neck:[90]

> the massive head, instead of being fully balanced on top of the column, was supported by a short, heavy, forwardly sloping neck, the bones of which recall the chimpanzee in having elongate neural spines. In association with the posture of the head and the relatively feeble development of the fore-part of the brain, the pre-pituitary plane of the skull was much less bent downward than it is in modern human skulls.

In accounting for these "facts," however, he was able to make use of Keith's functional interpretation of these characters for his own purposes. In 1914, Keith had theorized that the apparent lack of downward bending of the brain floor and the forward slope of the neck were both necessary to make room for the massive and deep Neanderthal lower jaw. In Gregory's view, it also made functional sense to view other "low" features of the Neanderthal skull -- the "strength of the ascending ramus" of the lower jaw, the "depth of the alveolar process", and "the great development of the supraorbital tori" -- as correlated with "taurodontism" and with the "powerful grinding action of the lower jaw." Additional "low" characters in the parts of the skull that articuated with the robust jaw could also perhaps be associated with the "rotary action of the mandible and with the edge to edge bite of the incisors."[91]

This functional interpretation of the Neanderthal head and neck regions made it possible for Gregory to come to a very different overall evaluation of the Neanderthals than either Boule or Keith, and one very similar to that of 1916. Rather than portray them as a specialized side branch, he could say that the "Neanderthaloids had reached the human grade of organization in most parts of the skeleton," while retaining "some important characters reminiscent of an early semi-erect heritage." The word "heritage," of course, was a loaded term in Gregory's vocabulary -- i.e. Neanderthal man's retention of "ape-like" characters was what one would have expected in a creature that had not undergone the final stages of "habitus" transformation into the skull form characteristic of <u>Homo sapiens</u>. This final stage, as he described it, was marked by the following correlated changes:[92]

> (a) forward growth of the cranium and deflection of the pre-pituitary plane (Keith), (b) the forward growth of the upper part of the face, and (c) the reduction in size and retraction of the jaws and dentition beneath the overhanging nose and forehead, which is so characteristic of the higher races of man.

Even though he had laid a solid groundwork for a "Neanderthaloid stage" in human evolution in these passages, when he actually discussed the "Cro-Magnons of western Europe" Gregory accepted the majority view, and stated that they "did not evolve out of the Neanderthaloids." On the other hand, he confessed that there was still "something to be said in favor of those (e.g. Hrdlicka) who regard the Neanderthaloids, or some of them, as structural ancestors -- primitive or perhaps archaic types surviving into a later epoch -- of the higher races."[93] This was obviously not a strong endorsement (and a partially misleading account) of Hrdlicka's views on Neanderthal man, and indeed, only tepid support for the "pre-Neanderthal" theory that he had stated so strongly in 1916. Still, the fact that the latter way of thinking was still congenial to him came through in his summary of "structural stages," which presented in the penultimate position a "low human type: Example: the Mousterian youth."[94]

Though Origin and Evolution of the Human Dentition brought several modifications in Gregory's analysis of specific fossil hominids, he added very little to his overall scenario of hominid evolution. In 1916 he had spoken only in a general way about the "correlation" between the adoption of the erect posture, the expansion of the brain, and changes in the hominid dentition, and said little about the difficult problem of describing and accounting for the relative rates of change in these three areas. The same was basically true again, but Gregory was a bit more forceful now in asserting that "the erect gait was assumed at a relatively early date" in human evolution, clearly because he now accepted the association of the "Pithecanthropus" skull and femur. This, when added to what he called "the thoroughly terrestrial rather than semi-arboreal characters of the Pleistocene Neanderthals and all later races," seemed to prove that the "early Hominidae or progressive Simiidae of late Pliocene or early Pleistocene age already walked erect upon the ground [Gregory's emphasis]."[95]

On the important question of the role of the brain in the transformation of the skull and dentition, a change in emphasis was perceptible as well. Brain expansion and changes in diet and food-getting strategies were still pictured by Gregory as proceeding *pari passu*. but in line with the influential views of Elliot Smith, the status of the brain as a causative factor was enhanced in the following passage:[96]

> Such a radical transformation of the jaw and dentition from a *Dryopithecus* -like type was correlated, apparently, with a pronounced change in food habits, from a prevailingly frugivorous to an omnivorous-carnivorous stage, and was even more intimately dependent upon a still greater transformation in the brain and braincase, from a primitive anthropoid to a human condition, which brought with it revolutionary disturbances and readjustments of the digestive apparatus and of the ductless gland complex that controls the growth and proportions of skeletal parts.

Gregory's further development of his dietary hypothesis, as well as his handling of the "Pithecanthropus" and Neanderthal problems, reveals a fundamental fact about American paleoanthropology in the 1920s that can easily be overlooked -- namely that there was little in the evidence as it then existed, even if one agreed with most of Boule's portrait of the Neanderthal skeleton, which could rule out a basically monophyletic interpretation of hominid evolution. The only real sticking point was Piltdown, and there were still enough questions associated with it that one could put it in limbo, at least provisionally. The major barriers to a unilinear scheme were really matters of underlying assumption -- the most important being scepticism of the idea that man was more closely related to the great apes than to any other primate, and belief in an early Pleistocene or late Pliocene appearance of anatomically modern Homo sapiens. Osborn, Frederick Wood Jones and Boule tended to subscribe to both, while Elliot Smith, Keith, Hooton, and to some extent, MacCurdy, accepted only the latter. Gregory, at least until the early 1920s, and Hrdlička, throughout his career, rejected both.

In a way, it seemed to be less problematical to accept or reject both assumptions at once than to accept only one. If one were inclined to believe in the anthropoid ancestry of humans one could easily discern, as Gregory did, that "a fair series of structural gradations" lay "already at hand, leading from the oldest lemuroid primates ... through Propliopithecus, Dryopithecus, Sivapithecus, Pithecanthropus, or through genera nearly allied to these, to the emergence of the Hominidae as a distinct family in Pliocene or early Pleistocene times."[97] And if this were the case, one could more easily carry this series through "Homo heidelbergensis" and "Homo neanderthalensis" than imagine hypothetical human ancestors who somehow lacked the "primitive" characters of these creatures, for it was just these "primitive" characters that seemed the best evidence of the transformation of anthropoid into human.

In using the word "assumptions" rather than "theories" here I am trying to suggest that matters of personal taste and general attitude toward man's relationship with the rest of the animal kingdom were just as important as inferences from existing data in determining a writer's degree of acceptance of dryopithecine ancestors and the geologically recent appearance of Homo sapiens. This is not to say that evidence was peripheral to the issue, however. Osborn et al. believed that the data confirmed their opinions, and

Gregory in particular devoted a great deal of energy to attacking this viewpoint. Such attacks, in fact, made up an important part of both Studies on the Evolution of the Primates and The Origin and Evolution of the Human Dentition. Before considering these critiques, however, it is necessary to examine Gregory's full development of his own theory, which was substantially completed in 1926 with his third major monograph on the primate dentition, The Dentition of Dryopithecus and the Origin of Man.[98]

The interim between 1921 and 1926, unlike the wartime period that had elapsed between Studies on the Evolution of the Primates and The Origin and Evolution of the Human Dentition, had produced some significant developments regarding Gregory's "dietary hypothesis." Ales Hrdlicka and the German scientist Adolf Remane (b. 1898) had each made detailed studies on the homologies between the teeth of pongids and those of humans; both, like Gregory, had stressed the resemblances between modern pongid and human teeth, and between both groups and the dryopithecines.[99] In addition there were new dryopithecine fossils from the Siwalik deposits of northern India in the possession of the American Museum to be analyzed. Also, though it is not overt in the Dentition of Dryopithecus itself, Gregory felt that his previous contributions had been slighted by Hrdlicka; this, plus the fact that the latter had generated a large amount of new data, could easily have caused Gregory to decide that a revised, detailed synthesis would be timely.[100] In the course of this re-evaluation the priority of his own hypotheses about the evolution of the human dentition could be established beyond doubt.

The possibility that Hrdlička's work had been a sort of spur for Gregory is strengthened by the new role that metrical data played in The Dentition of Dryopithecus. Hrdlicka had emphasized the need for careful measurement and sizeable samples in his recent writings, and now, for the first time, Gregory was also including multiple measurements of tooth dimensions and indices derived from the latter in his data base. He did this defensively, however, and without full confidence in the method, especially as it applied to fossil primates. As Gregory and his collaborator, Milo Hellman, noted in introducing the section that contained their new measurements, the degree of variation in tooth dimensions within each present-day species of anthropoid was great, while for the "few known fossils" there was "scant data for distinguishing individual from specific differences in measurements."[101] Also, they argued,[102] when one dealt with indices one's problems were compounded for

> each "index" in the tables gives only the proportion of length to width of an imaginary rectangle circumscribed around the tooth in a single plane, while the anteroposterior and transverse measurements themselves are necessarily taken in two different planes (Hellman). The resulting index gives no hint of the highly diverse forms and patterns that may be surrounded by the same rectangle.

Very slight errors in measurement, they also believed, could be magnified by indices, and finally when "comparing indices from an evolutionary standpoint," one had to remember that a "variable index may be raised either by increasing the numerator or decreasing the denominator." By contrast, asserted the authors, the "*Dryopithecus* pattern" had been clearly traced in modern anthropoids and in various modified forms in humans, an "indubitably well-established result" that had not been "reached by quibbling about differences in decimals, but by direct comparison of patterns and their parts."[102]

While these remarks should not be interpreted as a complete rejection of statistical methods, they clearly were a strong endorsement of traditional methods of evolutionary morphology -- i.e. the identification, description and comparison of two- or three-dimensional *forms* discerned by the trained eye. If larger samples of fossil material and statistical measures more sensitive to overall "shape" had been available, Gregory presumably would have made more use of and placed greater weight upon such methods. In the state of affairs that then prevailed, however, Gregory and Hellman were not unreasonable in allowing their inferences from the metrical data to be guided by hypotheses derived from traditional methods. This being so, it is not surprising that, despite an impressive accumulation of new data from specimens of dryopithecine lower jaws, from casts of fossil hominids, and from skulls of modern apes and humans, the judgements reached in The Dentition of Dryopithecus largely duplicated Gregory's previous conclusions.

The fundamental message conveyed was, then, that the dryopithecines were structurally ancestral to both modern pongids and humans. The lower molars, comprehensively analyzed both with regard to dimensions, indices, and variations on the "Y5" pattern, formed the central body of evidence (p. 55-82), but the details of incisor, premolar and upper molar form they discussed converged upon the same conclusion as well (p. 38-54). In addition, though the three newly-unearthed lower jaws from the Siwaliks were incomplete and ranged in age from mid-Miocene to Lower Pliocene, they seemed close enough in overall form to be merged into a composite reconstruction (p. 32-34).[103]

The resulting specimen of a "generalized" dryopithecine constituted a robust and deep mandible with "nearly parallel" tooth rows, similar in appearance to the already well-known European form Dryopithecus fontani. Though large and robust compared to humans, the jaw did not compare in size with those of large-bodied, modern anthropoids like the male gorilla, and did not possess the so-called "simian shelf" in the symphyseal region (where the two halves of the mandible join at the front) so common in modern pongids. These details of form meshed with the metrical data taken from the reconstructed jaw as well (indices such as the ratio of the width across the canines to that across the molars), allowing Gregory and Hellman to conclude that the lower jaws of "the most specialized anthropoids and man seemed to have evolved in opposite directions, both starting from the Dryopithecus stage."[104]

After they had established the various lines of evidence in support of the idea that the human dentition had developed from dryopithecine ancestors, Gregory and Hellman summed up the process that the human line had followed after "it had branched off from the Dryopithecus stem." In the anterior teeth -- incisors, canines, and premolars -- a "marked reduction" had occurred,[105] involving

> (a) an upward and backward movement of the crowns of the slightly procumbent incisors to a position of

> verticality, (b) a rapid diminution of the lower and upper canines, with eventual covering of the tip of the lower canines by the upper canines and lateral incisors, [and] (c) a rotation of the crown and roots of the anterior lower premolar from a more anteroposterior to a transverse position ... with a subsequent fusing of the roots.

In the molars, a relative widening of the first molar had taken place, they argued, along with a significant shortening of the second and third molars, "so that the first molar often becomes the dominant one" in modern humans. In the lower molars this change of shape had specifically involved "a shifting, differential growth, and realignment of the five main cusps ... involving especially the forward displacement" of one cusp, the entoconid. The result had been the "very gradual obliteration" of the well-known "*Dryopithecus* pattern" and its replacement with the "plus-shaped cruciform pattern" seen in modern humans.[106]

In previous works Gregory had associated changes in the teeth with, and had even made them partial causes of, transformations in other bones of the skull and jaw; in *The Dentition of Dryopithecus*, however, he and Hellman only hinted at such changes. At one point in their final summary they did, however, mention that part of the change from dryopithecine to human involved the development of the chin. While they advanced no hypotheses about why the chin had developed, they did point out that the human chin could have been derived easily from the symphyseal region of Dryopithecus, and also that intermediate stages existed in the jaws of various fossil hominids and in "lower modern human jaws."[107] They asserted that another important change from the dryopithecine to the human mandible had probably been a "great widening of the intercondylar diameter across the jaw;" this change they associated "partly with a great increase in the size of the brain and partly a great increase in the width of the tongue."[108]

The latter ideas represented nothing new; from 1916 on Gregory always pictured the expansion of the hominid brain as closely correlated with the transformation of the dentition. In the final sentence of their conclusion, though, Gregory and Hellman did add something about the brain that constituted a small but meaningful change of emphasis from Gregory's earlier writings -- namely, that the "many changes in the jaws and teeth" which they were discussing had "accompanied or lagged behind the great expansion of the prefrontal, parietal and temporal lobes of the brain and the development of the highly mobile tongue and articulate speech."[109] The key phrase was "or lagged behind," for it seemed to denote a further concession to the brain-centered view of human evolution defended by Elliot Smith, Keith and other proponents of

Piltdown man.

How much of a concession Gregory was making is hard to judge, for significantly, very little was said in <u>The Dentition of Dryopithecus</u> about the later stages of hominid evolution. Data from fossil hominids was used to document the process of transformation in individual teeth, but not in the passages that dealt with the evolution of the skull and dentition as a whole. That Gregory's retreat from the scenario he had staked out in 1916 was of importance is suggested by one of the illustrations included in the monograph, however -- a photograph of a display in the American Museum's "Hall of the Age of Man" representing "The Ascent of Man from the Lower Primates, As Inferred by William K. Gregory, 1924." The display, which depicted various reconstructed primate skulls arranged on a family tree, labelled the first skull on the hominid line after its divergence from the chimpanzee-gorilla group as an unspecified member of the genus <u>Dryopithecus,</u> as one might have expected. However, the next most ancient skulls, those representing the "Trinil Ape-Man" and the "Piltdown Man," were on short stems leading off the main branch, indicating that they were close approximations to actual human ancestors. Surprisingly, on a longer side-branch (and one that split off the stem after "Trinil" and <u>before</u> Piltdown) were the skulls of both Heidelberg and Neanderthal man.[110]

A "family tree" of this sort obviously implied a very different picture of hominid relationships from the one Gregory had outlined in 1920 and especially 1916. The consequences that it might hold for his "dietary hypothesis" were not explored in the text; perhaps a reluctance to develop its implications accounts in part for the scant discussion of fossil hominids in The Dentition of Dryopithecus. In fact, indications that Gregory was uncomfortable with his reversal on the relative claims of Heidelberg man and "Eoanthropus" were present in a passage dealing with another issue he had changed his mind about -- the relative proximity of various dryopithecine species to later hominoid genera.

In Gregory's first two monographs on the human dentition the discussion of the latter issue had been a bit confusing. At one point the Siwalik genus Sivapithecus had been treated as a representative of the conjectured "early dryopithecine stage" in the hominid dentition, while the various species of Dryopithecus were seen as having closer connections with the chimpanzee and gorilla. In another passage, however, Gregory had placed Sivapithecus in the status of an "early offshoot" of the dryopithecine radiation, intermediate in form between the line leading through Dryopithecus itself to modern African apes and modern humans and the line through the Siwalik genus Paleosimia to the modern orang.[111] In 1926, however, Gregory and Hellman speculated in their analysis of molar form that the close resemblances among Sivapithecus, Paleopithecus (a precursor of the orang), and one of their new Siwalik fossils, Dryopithecus frickae, led to a different conclusion -- namely that "on the whole the Indian 'Dryopithecus' seems to be allied rather with the orang than with the gorilla - chimpanzee - man group, the former constituting an eastern, the latter a western division of the family Simiidae."[112]

One of the important members of the so-called "western division" was the European fossil Dryopithecus rhenanus, and in discussing its molars Gregory and Hellman made the following revealing remarks:[113]

> In D. rhenanus the upper and lower molars, as noted by previous observers, approach the chimpanzee type. Some of the molars referred to D. rhenanus also suggest the human type. ... The Piltdown jaw mingles the characters of Dryopithecus, the chimpanzee, and early man, and if it really belongs to the Piltdown skull, then there can be no doubt of the close relation of these three genera. But even if the Piltdown jaw be not human the human cheek teeth are on the whole distinctly nearer to those of the chimpanzee than to those of the gorilla, so that D. rhenanus may stand near the point of divergence between chimp and man.

These words (in addition to erecting major phylogenetic speculations on sketchy evidence) indicated a continuing hesitance toward "Eoanthropus" that was plainly at odds with the prominent role Gregory had given it in his hypothetical family tree. Still, the fact that he could find a use for the Piltdown jaw should it have turned out to be that of a hominid shows how the parts of Gregory's theory dealing with the earlier phases of human emergence could be maintained despite the confusion that the Piltdown fraud caused for his interpretation of the later stages. Half a theory was apparently better than none, especially when the concepts of a dryopithecine ancestry for humankind and of a close evolutionary relationship between humans and African great apes were the half that was of paramount importance to the discoverer of the " *Dryopithecus* pattern."

Defense of the "Ape-Man" -- 1914-1925

From the perspective of the present day -- when chimpanzees and gorillas apparently learn sign language, when genetic studies reveal amazing similarities betwen chimpanzees and humans, and when fossils like "Lucy" display bipedal locomotor adaptations in association with pongid-like dental characters -- the hypotheses that our closest relatives are the African great apes and that we shared a common ancestor with them as late as the second half of the Miocene epoch are hardly exciting. In the period between 1915 and 1935 contrasting views to this one about primate relationships and about the age of a distinct hominid line were defended vigorously, and even fanatically, by their adherents. Osborn's was only one of those with which Gregory was in conflict, and the latter found it necessary at every stage of his own studies to criticize competing hypotheses. Indeed, once his basic framework was completed by The Dentition of Dryopithecus, most of Gregory's writings on primate evolution prior to his work on the australopithecine dentition in the late 1930s concerned themselves with the task of questioning the evidence and assumptions of these opponents.

Gregory's first defence of what he would come to call the "ape-man" theory actually appeared before he had advanced his first version of the latter, in a brief 1914 report on a discovery that was to cause him so many headaches in the future -- Piltdown Man. In this report he did not promote any particular interpretation of the fossil, but rather tried to forestall any speculation along unproductive lines. Specifically he cautioned against the view that the smooth forehead and human type of brain represented by the Piltdown cranium disproved the "Darwinian idea of human descent from an 'anthropomorphous' ape."[114] In the early stages of

Piltdown interpretation Gregory's position was easy to defend, for the "ape-like" jaw had not yet been questioned by Miller, and Smith Woodward's early reconstruction of the braincase had yielded a small (1060 cc.) endocranial cast in which Elliot Smith had found several "primitive" characters.[115]

The comparative anatomist in Gregory also counselled against too great a reliance on fragmentary fossils. In sentences he might have criticized had Osborn written them in 1930, he asserted: "the proof of the ascent of man from certain still-undiscovered mid-Tertiary primitive apes does not rest largely upon the scant fossil remains of extinct races of men and apes. It does rest upon the convergence of many lines of evidence offered by the embryology, anatomy and fossil history of numerous races of animals."[116] In concession to those who felt that the dignity of humankind was undermined by its possession of "ape" ancestors, he added that though human faculties had evolved from those of anthropoids, "even the lowest existing races of mankind are extremely superior to apes in mentality, in power of speech and in ability to use the hand as an organ of will and intellect." The time required for the production of the human race had thus been a long one, and the truly "primitive" Piltdown specimen represented a sort of "man in the making ... an early branch of the prehuman stock."[117]

In Studies on the Evolution of the Primates the interpretation of Piltdown man was a major problem for Gregory, but this time it was joined by another issue -- the relative limb proportions of hominids and other primates. His discussion of both was in response to the ideas of Marcellin Boule, who in the decade between 1910 and 1920 was the most prestigious critic of the "ape-man" theory. The question of limb proportions had assumed importance because of the prominent role Boule had given it in his writings on the Neanderthal skeleton. Boule had made his analysis of the La Chapelle aux Saints skeleton the occasion for a comparative study of the limb bones of humans, apes, Old World monkeys and lemuroids. His conclusion, derived from the relative proportions of the various limb segments, was that man shared crucial features with Old World monkeys that he did not share with the great apes. Conversely Boule had argued that similarities which did exist between man and the latter were products of convergent evolution and not indicators of recent shared ancestors.[118]

All of this had come before the appearance of the Piltdown fossils, and the capacious Piltdown braincase had encouraged Boule to speculate further along the same lines in 1915. Though he saw problems in associating the skull and jaw, he did accept the braincase as that of a true representative of fossil man. Its form, he believed, supported the view that the low-vaulted crania with projecting brow ridges of "Pithecanthropus" and the Neanderthals were not primitive characters for the Hominidae, but were later specializations convergent with similar characters in the great apes. Boule also contended that the name "Eoanthropus" was a misnomer for a creature so recent in geological age and advanced in brain size. When the " véritable Eoanthropus," the true ancestral hominid of Eocene age was found, it would bear a relation to humans similar to that which Eohippus, the ancestor of the horse family, bore to modern equines. In Gregory's characterization of it, Boule's Eocene hominid would have been a sort of "homunculus" -- a creature "of small stature and almost erect posture, with a brain case very voluminous in relation to the total volume of the body, but very inferior in value to that of all the Hominidae now known."[119]

The conception that Boule had developed of hominid emergence was, of course, diametrically opposed to the one Gregory was advancing in Studies on the Evolution of the Primates; though he treated Boule's ideas with respect, he also tried to subject them to vigorous criticism. "Homo Dawsoni" (Boule's term for the Piltdown skull), for one thing, could reveal little about the skull form of the earliest hominids, precisely because it was too recent in age. The picture of the "true Eoanthropus," argued Gregory, also relied excessively on the principle of recapitulation; believers in homunculi put too much store in the "swollen head of the human fetus" as an indication that man's ancestors could not have had low, retreating foreheads. Rather than evidence of man's ancestral "heritage," the large brain of the human fetus was a "caenogenetic character" -- i.e. a recent acquisition correlated with the large size that the adult human brain had reached in the later stages of hominid evolution.[120] In addition, in an earlier passage, Gregory had put great stress on size, robustness and ferocity as adaptive characters in early hunting and meat eating hominids; if this scenario were accurate the idea of a "homunculus" was quite unlikely.[121]

The question of limb proportions was more difficult, but Gregory argued that here as well the evidence did not support the inferences Boule was trying to make. A basic objection could be raised against the relevance of the data itself. After all, the American noted, "all M. Boule's elaborate discussion of limb ratios and indices rests upon a comparison of recent [emphasis Gregory's] anthropoids with recent and Pleistocene Hominidae." What

Boule really required was skeletons of Upper Miocene hominoids to see whether the ancestors of modern pongids and hominids were truly so distinct from one another.

Also, a consideration of "habitus" versus "heritage" called into question Boule's interpretation of the data on recent species. The key fact for Gregory was man's assumption of the erect bipedal "habitus," a change which had "involved adjustments and reversals in the proportional lengths of the limb segments -- readjustments, of which the true significance has largely been missed by those who put their trust in ratios and indices."[122] That humans as a result of these specializations had come to possess relatively much longer legs and shorter arms than their pongid cousins posed no problem, for[123]

> both the long femur and long tibia of man greatly lengthen the stride and increase the speed, factors of vital importance in a hunting and fighting animal, but of less importance to the clumsy, frugivorous anthropoids. The short arms in man are also more powerful and of greater advantage in fighting with weapons. On the other hand ... long legs and short arms in the tree living anthropids would be inconsistent with the fully upright posture in sitting and with the habit of brachiation.

The objection that one "brachiator," the gibbon, actually had longer legs in relation to trunk length than the chimpanzee and gorilla did not, Gregory felt, disprove his point, for gibbon-like hind limbs would not have worked for "a heavy animal like a full grown chimpanzee or gorilla, since they would make it more difficult to maintain the balance."[124] On the whole then, the differences in limb proportion and in other structures involved in locomotion such as the foot could be explained as adaptations to differing "habitus;" they could not, in Gregory's view be used to undermine the evidence "from the anatomy of the brain, genito-urinary organs and countless other structural and physiological resemblances," evidence which indicated "with practical certainty" the extremely close evolutionary relationship between humans and the existing pongid species.[125]

Conversely, the similarities in proportion between humans and certain monkeys and lemuroids that Boule had discussed could also be accounted for, but not as evidences of close genetic relationship. Thus, an arboreal lemuroid like Galago might have relatively longer legs and shorter arms than an arboreal ape, but it had these proportions because they were "specialized for leaping in a manner entirely different from the erect bipedal progression of man," or the brachiation of the chimpanzee. Similarly, while in some of the "cynomorph monkeys the disproportion in length between arms and legs is less than it is in the anthropoids," this characteristic existed "precisely because they are quadrupeds, and they walk upright only with the greatest difficulty." Though they could sit "partly upright," the monkeys' skull, backbone and pelvis were "far less like man's than those of the great apes."[126] It was thus these resemblances between humans and non-hominoid primates, he believed, that were accidental products of parallel evolution.

Even at this early stage in his involvement in the debate over hominid relationships Gregory realized that theoretical assumptions as well as specific bits of evidence lay behind objections to the "ape-man" theory. Adherence to the recapitulation theory is one instance that has been noted above; another that he discussed was the tendency to "expect remote ancestral stages to foreshadow all [Gregory's emphasis] the features of the final stage" in an evolutionary line.[127] These words actually described the practical result of invoking the two principles of orthogenesis and irreversibility of evolution, principles which, as we have seen,[128] were to play a critical role in Osborn's "pro-Dawn man" theory.

While Gregory did not criticize these principles, or the "biogenetic law," in detail in 1916, he did sound a note that would recur throughout his more detailed discussions of theory in the 1920s and 1930s -- i.e. that transformations in organic form, and specifically reversals in evolutionary trends, can and do occur, and that such changes accompany changes in function and adaptation. Thus, he asserted, if one were willing to "admit that the trend of evolution sometimes changes, following a change of habits," then one could find "plenty of precedence for the reduction of one part and the increase in another" that he believed to have produced the hominid locomotor apparatus. Most evolutionists accepted the theory that the change from terrestrial to aquatic life had produced a "profound readjustment of proportions" in the limbs of Pinnipedia (the group to which modern seals belong). Why could a similar, if less profound, transformation in locomotor habitus not have done something similar to an arboreal, brachiating anthropoid embarking upon a career of terrestrial bipedalism?[129]

In The Origin and Evolution of the Human Dentition Gregory aimed similar criticisms against the opposition to those he had advanced in 1916. This time, however, he did not single out any other author for rebuttal but rather focussed his attention more broadly on the major ideas he felt were held in common by those who rejected "ape-like" human ancestors. The first, which we have already encountered was the confidence that many resemblances between present-day apes and humans could be accounted for by invoking "convergence" and "homoplastic (parallel) evolution." Second, these writers seemed to him willing, and even eager, to believe that the so-called "mystery of man's origin" was still far from solution, owing to the "incompleteness of the geological and paleontological record."[130] Though on the latter view the early hominids were still completely unknown, Gregory also noted how those who disagreed with the "Darwinian" view showed great confidence that, when and if such "early Tertiary" human ancestors were found they would be "large-brained" creatures. This confidence, he thought, was based on two mistakes (ones which he had alluded to in 1916) -- first "the too prevalent fallacy that remote ancestral stages must already foreshadow all the characters of their distant descendants," and second the "naive faith in the biogenetic law" from which came the inference that "the swollen brains of young stages are reminiscent of adult brain form of ancestral stages."[131]

Gregory's final criticism stemmed from his observation that those who opposed the transformation of anthropoids into hominids often based their arguments on analogies with "the many well-known cases involving [instead] an intensification of given functions and further progress in the same direction as in earlier stages." This process had often occurred in animals that had remained "in the same environmental zone" -- a prime example being the plains-living ungulates, which had "simply improved their mode of locomotion and dental apparatus without radically altering the plan of them." Such cases "being numerous and well-known were apt to be taken as the standard examples of the way that evolution normally" proceeded; from thence, cautioned Gregory, might "arise the unconscious impression that nature is limited to that kind of 'orthogenetic evolution',"[132] which to his mind it clearly was not.

Just as this portrayal of the opposition made extensive use of ideas first broached in 1916, so also did Gregory's response to it. First, that so many similarities between pongids and hominids could be the result of convergent and parallel evolution taxed one's credulity, he asserted, especially as there were no fossils of the "true" human ancestors to support the hypothesis. The absence of fossils was a crucial problem in itself, since the theory demanded "a long series of genera and species ... ranging from the lower Eocene onward;" it seemed odd that apes and "ape-men" had been found but not a "trace" of these primitive hominids.[133] For his part, Gregory confessed that he had to and did "accept the paleontological record much as it stands ... there is no necessity for postulating the existence of Eocene Hominidae as a family distinct from the Simiidae."[134]

In the existing state of the evidence there seemed to be to him no convincing alternative to the theory that humans were ultimately derived from the transformation of an arboreal, "brachiating" anthropoid into a ground-dwelling bipedal hominid, with a central role in the change being played by alterations in food habits. To those who objected that such a transformation violated the principle of "orthogenesis" Gregory replied that "all the great evolutionary advances, as when tetrapods evolved from fishes, or when mammals evolved from reptiles, have been revolutionary in character, since they involved profound changes and readaptations in the methods of locomotion and feeding [Gregory's emphasis]".[135] One did not need to look so far back for such transformations, either. The record of mammalian evolution in the latter part of the Tertiary, the period when hominids would have been evolving from a dryopithecine ancestor, contained analogous examples of shifts in locomotor and feeding habits correlated with major morphological transformations in groups as widely separated as whales and sloths.[136]

Finally, to the oft-raised objection that the time span between generalized dryopithecines and humans was too short to have allowed for the massive growth of the brain required, Gregory replied that the span from mid-Miocene to lower Pleistocene had traditionally been underestimated. The best estimates available now indicated that the period involved might be "more than a million years," a period "long enough perhaps for the brain to expand from an anthropoid to a low human stage."[137] Also, while the gap between pongid and human brains and behavior was admittedly immense, the most recent work in both comparative neurology and psychology seemed to indicate to him that the differences were ones of degree, not of kind, a fact which made the theory of transformation in the later Tertiary even more plausible.[138]

When combined with his positive evidence for a close relationship between the great apes and man based upon the dentition, Gregory's arguments against alternative views seemed formidable. Still, it should not be a surprise to find that Gregory's critique failed to forestall further speculations about "Dawn men." The evolution of the brain was still obviously a sticking point, even for those sympathetic to the "ape-man" theory. Thus, Sir Arthur Keith, while fully in agreement that the orang, gorilla and chimp were man's closest relatives, still could not accept a mid-Miocene dryopithecine as their last common ancestor. Allowing seven generations to a century, and taking Gregory's estimate of about one million years from the mid-Miocene to the lower Pleistocene, Keith confessed that he could not "conceive the possibility of the extreme structural and functional complexity of the human brain having been evolved from the anthropoid stage in the course of 70,000 generations."[139]

If Keith was unwilling to believe that the brain "arose as a mushroom-like growth"[140] this view applied _a fortiori_ to the defenders of the _homunculus_ idea. As Gregory also knew the latter would continue to appeal to "laws and principles" of evolution that had acquired considerable prestige over the years. In addition, the notion that the human line had been evolving separately from those of other primate groups conformed to long-cherished traditions affirming humankind's uniqueness in the animal kingdom, traditions that were highly resistant to criticism. Finally, there were legitimate empirical doubts that could be raised about the sufficiency of the comparative anatomical and paleontological data upon which the "Keith-Gregory" theory of hominid emergence rested.

For all these reasons the question of whether humans shared a recent ancestor with the great apes remained a "hot issue" in the anthropological world of the 1920s in a way that is perhaps hard to imagine today. Not only did the meaning of the existing data continue to be debated, but new studies were undertaken which their initiators hoped would help point out a solution. The Dentition of Dryopithecus is an obvious instance of the latter, but in the United States one must also count Hrdlicka's studies on the dentition, Adolph Schultz' work on comparative primate embryology, Dudley J. Morton's pioneering biomechanical studies on the primate foot, and Frederick Tilney's analyses of the primate brain stem.[141] In England the lines of debate ranged Keith and Elliot Smith against Smith's pupil Frederic Wood Jones. In fact Wood Jones became, with Osborn, the principal supporter of the Eocene "Dawn Man" and as such drew an increasing share of Gregory's criticism as the debate progressed.[142]

As we have seen above, it was not merely a lack of acquaintance with the evidence that in Gregory's view caused scientists to reject "ape-like" human ancestors, but also overreliance on certain principles of evolution. So limiting were these principles, he believed, that those who held to them firmly "would fail to recognize a direct ancestor of man of Miocene age even if it were represented by a complete skeleton, since they would expect to find it abounding in the diagnostic characters of recent Hominidae and to be widely different from the contemporary Simiidae [Gregory's emphasis]."[143] It is not surprising, then, that Gregory would try to subject these principles to a direct critique, or that his first target would be the venerable "biogenetic law" or principle of recapitulation.[144]

In his discussion of the relative value of the "biogenetic law" to the interpretation of the skull form and dentition of fossil hominids, Gregory began with the more general problem of the pitfalls one encountered if one tried to use it as a universal principle in vertebrate paleontology. Thus, examination of several groups of mammals where a relatively full fossil record already existed, such as the cat family, revealed that fossilized adult forms did not approximate the fetal or youthful forms of present-day species in quite a few important skeletal characters. Would it be sensible, he asked, to rule out all of these "nonconformist" fossils as possible ancestors of present species, and substitute chimerical "generalized" forms compatible with the biogenetic "law" instead? Not only would it be a mistake to expect fossil adults always to duplicate the fetal characters of present species, but it also seemed to him an obvious fact that many characters of modern fetal and youthful mammals are adaptations to the conditions of life encountered by the young, and could never have been characteristic of any adult ancestor. To take the simplest example, clearly no mammal had ever lived on maternal milk throughout its life. Gregory made no claim that these observations were in any way original, only that they strongly supported the conclusion that "in each instance the supposition that a given ontogenetic character is primitive [derived from adult ancestors] requires independent evidence" of the sort provided by the fossil record itself.[145]

Gregory's other objection to relying on the recapitulation theory grew out of von Baer's famous principle that the stages of ontogeny were ultimately regulated by the pressure to produce "viable" or well-adapted adult animals. This "final cause" built into the developmental process guaranteed that many fetal characters would be "anticipatory" characters rather than "reminiscent" of ancestral ones. When applied to the primates von Baer's principle could easily explain conditions like the "bulging forehead" of human and ape fetuses, since the high intelligence relative to other animals that had evolved in <u>both</u> groups seemed to require accelerated brain development. Reaching large brain size early in ontogeny, he thought, represented a clear "preparation for the process of building up one reaction after another and establishing more and more complex connections between the multitudinous [nerve] centers" that the acquisition of higher degrees of intelligence required.[146]

Finally, Gregory called attention to the specific details of primate paleontology and comparative anatomy to illustrate his case. First he pointed out that the earliest fossil primates so far uncovered, of which the Eocene lemuroid Notharctus could serve as a representative, possessed many "generalized" skeletal characters that one might expect to find in ancestral primates, while their skull form in no way approached the fetal form of present day higher primate species. Just as damaging, he thought, were the generally accepted conclusions of Elliot Smith on the course of evolution of the primate brain and skull. For if brain expansion had gone hand in hand with a shifting forward of the eyes to achieve stereoscopic vision, the appearance of brow ridges in great apes and the most ancient forms of fossil man could best be explained as a protective adjustment to the forward position of the orbits. Only with the final expansion of the brain case over the orbits and the retraction of the dentition in later hominids would these ridges have disappeared. The fact that modern fetal primates lacked brow ridges thus completely failed to prove that this character was a specialization ruling out "beetle browed" hominids and fossil apes as potential human ancestors.[147]

Gregory capped off his critique by returning to the familiar domain of the dentition. If, as he was certain, the Dryopithecus molar pattern showed that humans and great apes shared a Miocene hominoid ancestor, then how was that creature to be reconstructed? Since the teeth of modern day apes and of fossil humans differed less from the ancestral pattern than did those of recent Homo, it seemed reasonable to conceive of their Miocene ancestors as possessing a set of cranial characters that would correlate somehow with robust dentitions. Their skulls would thus have been likely to display the well-developed brow ridges and prominent supports for nuchal and masticatory muscles that characterized both modern pongids and early hominids like "Pithecanthropus." Given the fact that the ancestral dryopithecine dentition was more robust than that of the earliest known hominids, "Pithecanthropus" and "Eoanthropus," it also made sense to picture the first hominids as more "primitive" in general skull form than either fossil. One could then easily derive all the known fossil human types from this hypothetical ancestral stock -- with some branches like "Eoanthropus" and Homo sapiens "progressive" in the form of the skull vault and others like "Pithecanthropus" and the Neanderthaloids labelled as "conservative."[148]

With the last link of his chain of reasoning in place, Gregory had achieved his short term objective -- to undercut the practice of manufacturing hypothetical large-brained Eocene hominids on the basis of "evolutionary principles" alone. This gain, however, was achieved at a price, for along the way Gregory had fully conceded the notion of "parallel phyla" during later stages of hominid evolution, and thus by 1925 had left Hrdlicka as the only major writer in America still promoting a unilinear theory of human emergence.

While Gregory's course had considerable logic to recommend it given the character of the Piltdown "evidence," it unfortunately destroyed the neat correlation among the factors of primate morphology, mode of adaptation and geological time that had distinguished his original scenario of human evolution in Studies on the Evolution of the Primates. The crucial differences in this regard between 1916 and 1925 were two -- 1) by accepting all of the Piltdown remains as "Eoanthropus" one lost the steady reduction of teeth and jaws in tandem with the evolution of "omnivorousness" and tool use that had characterized the Heidelberg man - Neanderthal man- anatomically modern Homo sapiens sequence; and 2) even though Piltdown man was relatively thick-skulled, and contained an endocranial cast that had been read as suitably "primitive" in form, the skull had none of the exterior ruggedness that Gregory had earlier claimed to be necessary in predatory hominids living without the benefit of a developed culture. In admitting the association of the Piltdown jaw and skull, Gregory was negating his own principles of reconstruction of ape and hominid fossils -- i.e. if ape-like dentitions on basically smooth-skulled creatures were permissible in the early Pleistocene, why could they not have been so in the mid-Miocene?

The Debates With Osborn and Wood Jones

The continued vitality of the "Dawn Man" theory did not have a great deal to do with technical weaknesses in Gregory's arguments against it, however. Given the paucity of fossil data on pre-Pleistocene higher primates all sorts of hypothetical hominid ancestors were possible. Those who, like Osborn, made much of Piltdown Man and "eoliths," or like Frederic Wood Jones, saw revealing "generalized" characters in both humans and non-anthropoid primates, could extrapolate the "Dawn Man" as far back as they wished. The only effective reply that defenders of the "ape-man" could make had to rely on the weight of the evidence from primate paleontology and comparative anatomy and psychology. As Gregory did the most to point out where the balance of existing evidence lay, as well as to explore weakneses in the assumptions used by the "homunculus" theorists, he can be credited with doing more than any other scientist in the period to define "orthodox" opinion on the degree of relationship between humans and other anthropoids. Though the theory of a "brachiating" anthropoid ancestor continued to be questioned, by the mid-1930s Eocene "Dawn men" were definitely passe in America, and it seems likely that this "ground clearing" operation was very influential in insuring a favorable reception in the United States for fossils like Peking Man and *Australopithecus.*

As we have seen,[149] Osborn's dissatisfaction with the "ape-man" theory developed relatively slowly, finally becoming a full critique in the late 1920s. Not surprisingly, Gregory's opposition to Osborn's new ideas followed the same pattern. Early signs of divergence were visible in a joint effort by these authors to call attention to the main "facts" about human evolution in 1925 -- i.e. in a revised guide to exhibits in the "Hall of the Age of Man" at the American Museum. The text of the booklet, which was largely by Osborn, laid down the outlines of human prehistory and attempted to describe the life ways of the principal forms of Paleolithic and Neolithic humanity; in general it merely recapitulated the contents of *Men of the Old Stone Age.* Two references to Osborn's newer views occurred though: first was the claim that "Foxhall Man" of Reid Moir was the first undoubted example of "Tertiary" man so far known (known of course by his "implements" and not by skeletal remains); second, regarding earlier hominid forms, Osborn asserted that "the ancestors of man lived partly among the trees and partly among the forests, [but] this does not mean they were arboreal; they lived chiefly on the ground."[150]

As editor of the pamphlet, Gregory apparently could not let the latter statement pass unmolested. In a footnote he informed the reader that the passage referred "only to the higher, more recent ancestors of man." Recent studies on the primate foot, he added (undoubtedly referring to the work of Morton), tended to support "the view that the human foot has been derived from an earlier ape-like stage in which the great toe could be used in climbing."[151] An "arboreal ape" stage in human evolution thus seemed essential. Reflecting this stress on the anthropoid heritage of man as well was the appendix that Gregory provided on the Hall's comparative anatomical exhibits. There the great apes, fossil hominids and modern varieties of Homo sapiens were arranged in graded sequences in such characters as skull form, size and shape of the mandible and brain size. While direct ancestry was not indicated, closeness of genetic relationship clearly was being asserted.[152]

When in 1927 Osborn launched his series of essays and addresses against the "ape-man" and in favor of his "Pro-Dawn" man, Gregory undertook a vigorous counteroffensive which brought the differences between master and protege out of the footnotes. Between 1927 and 1930 Gregory published half a dozen articles critical of Osborn's theories in major periodicals.[153] Osborn, for his part, showed no resentment and even shared a platform with his junior colleague for a friendly debate on human origins. Despite the genial personal aspect of the debate, Gregory's critique of Osborn was highly effective intellectually; it left the latter with little more than a priori hypotheses to support his argument and even called these into question.

Comparative anatomy provided the bulk of the evidence by which Gregory tried to show that the great apes and humans shared too many unique characteristics to support the hypothesis of "parallel evolution" from a remote lemur-like or monkey-like ancestor. Using the morphology of the humerus and the hand to provide details, he argued that the pectoral limb of man was a "vertible palimpsest" revealing beneath its adaptations for manipulating objects evidence of "an earlier period when every bone and muscle was adapted for the habit of supporting the body weight by the uplifted arms."[154]

Similarly he contended that the human foot and pelvis, now so well designed for bipedal locomotion, showed clear signs not of having developed from primitive primate forms independently, but of having passed through an ape-like stage first -- a stage in which the pelvis had undergone an initial broadening and the foot had been a "biramous," grasping organ with its weight bearing axis lying between the first and second digits.[155] Indeed, the then recent research of Morton on the foot structure of the mountain gorilla, the least arboreal of the African great apes, revealed such similarities to the human foot that Gregory could see no plausible alternative to the belief in a recent common ancestor.[156]

Though he had not done research on them personally, Gregory could point to supporting evidence from several other parts of the anatomy. Keith, he asserted, had shown how the internal arrangement of the viscera in brachiators not only departed from conditions encountered in lemurs and monkeys but also agreed fundamentally with the human pattern. Elliot Smith and his students in England, and more recently Tilney in America had revealed a qualitative near-identity between the brain structures of great apes and humans; the most primitive, the gibbon, and the most advanced, Homo sapiens, were separated largely by quantitative differences in the size of cortical association areas, with the larger apes and the primitive hominids bridging the gap by degrees.[157] And of course in the field in which he had done exhaustive research, the evolution of the teeth, he saw no reason to modify previous conclusions. In particular, he reiterated his conviction that the derivation of the human molar from a dryopithecine ancestor was a "transformation which can be doubted only by those to whom morphological evidence makes no appeal."[158]

Though they were not in his field of expertise, Gregory could also draw support for his morphological argument from then recent findings and speculations by comparative psychologists like Wolfgang Köhler and Robert M. Yerkes. The general tendencies of these studies was supportive of the notion that the great apes were more closely related to humans than was any other animal group, and displayed problem solving abilities similar in kind to human ones.[159] This psychological similarity was exactly what might be expected given Tilney's argument, which Gregory endorsed, that apes and humans represented the highest development in the animal kingdom of the power of "neokinesis" -- i.e. the ability to guide behavior by complex cortical associations rather than by the more primitive reflexes and instincts mediated by the lower brain centers.[160]

While comparative anatomy provided the main support for Gregory's critique, he also tried to deny Osborn the comfort that the latter had tried to derive from embryology. It will be remembered that Osborn had made much of Adolph Schultz' observation that the hands of human fetuses showed no signs of a stage reminiscent of the "limb grasping" specializations of modern apes, as the biogenetic law seemed to demand if humans had had "brachiating" ancestors. Gregory replied to this with a variant of the von Baerian principle that he had invoked in 1925 -- namely, that the more important finding by Schultz was the fact that both fetal humans and fetal great apes possessed a short, wide hand from which the divergent adult specializations found in each could readily develop.

In regard to the foot, Osborn had claimed that the "prehensile big toe" of the human fetus could just as easily be a reminiscence of an Eocene primitive mammal ancestor as of a Miocene hominoid. Gregory countered again with a closer look at Schultz' data; the latter had found a series of stages in the prenatal development of the foot in humans which closely mirrored those that would be necessary to transform an adult "gorilloid" form into a human one. Here indeed, in Gregory's view, was an instance of recapitulation -- though he continued to argue that all organs did not have to obey the "biogenetic law," he could, and did express pleasure at the confirmation for his theory provided by the organs that did so.[161]

Gregory's third line of attack versus Osborn was more purely concerned with theory than those above, since he rightly saw that much of Osborn's case rested on certain supposed "general laws" of evolution. As Gregory saw the problem, the first principle involved was that of "polyphyletism" -- the belief that each order of mammmals "includes a large number of genera which may be traced backwards along independent lines through amazingly long periods of geologic time."[162] Second was the theory (which Gregory termed "orthogenetic specialization") that each such phylum displayed a set of characters, especially in the skeleton and teeth, adapting it for some "special mode of life" or niche; once such an adaptive complex was achieved the direction of evolution within the phylum was allegedly toward increasing specialization along the same line. In the younger scientist's analysis of it, "orthogenetic specialization" seemed to be compounded of three ideas: 1) a strict version of Dollo's rule of the "irreversibility" of evolution which decreed that specializations once perfected could not be cast aside, 2) the belief that the potential for variation was severely limited at each particular stage of evolution, and 3) the theory that any ancestor must have had an ensemble of characters which in sum possessed the "potentiality" for developing into the patterns evinced in all of its descendants.[163]

If these principles had indeed been universal ones, the path of human evolution that Osborn had hypothesized would have made a great deal of sense, especially if one accepted the existence of large-brained species of Homo in the early Pleistocene. However, just as he had done in his earlier discussions of "homunculus" theories, Gregory insisted that adaptive reorientation and morphological "transformation" were just as important principles in vertebrate evolution as those that Osborn was defending, and he drove home the evidence for his contention in much greater detail than he had before. Taking the broadest view of the issue open to him, he underlined his belief that most of the key "structural stages" on the evolutionary path from "fish to man" had involved shifts in locomotor patterns, food habits, etc., and that these had clearly involved new specializations rather than merely the perfecting of old ones.[164] Among marsupial mammals the multiplicity of forms that had developed in Australia could also not be explained without invoking profound transformations in both life habits and morphology; change of similar magnitude had obviously occurred during the evolution of whales from terrestrial carnivores among the placental mammals.[165]

Nor was data lacking to reveal hominid evolution as a process involving such a transformation, asserted Gregory, were one disposed to look for it. If for example one examined the teeth, especially the molars and premolars, of pongids and hominids one would find a high degree of variability and considerable overlap in form; these conditions would not be found, though, in a comparison of groups like tapirs and horses, which conformed to the pattern of "orthogenetic" change on long separated evolutionary lines. The hominoid condition seemed to Gregory (as it did to Hrdlicka also) to be clear evidence of "heredity instability and rapid evolutional divergence." On this point the confusion engendered by acceptance of the Piltdown teeth could be turned to special advantage -- since the ape-like Piltdown molars were "almost indistinguishble from those of the anthropid Dryopithecus, while the Heidelberg molars are distinctly human in pattern."[166]

If one chose to insist, in the face of such evidence, that the rate of evolution in the primates must be the same as in other mammalian groups such as equids or proboscideans, the argument for an upright-walking Eocene or early Oligocene hominid still seemed weak to Gregory.

Compared to groups which had diverged in the Eocene, again like tapirs and horses, the overall skeletal and dental differences between humans and pongids were not very great. Moreover, the features in which humans differed significantly from chimpanzees, for example, were precisely those which were obviously related to their divergent adaptive patterns -- that is, they were the so-called "habitus" characters that he had been discussing since 1916. He still had to be convinced that humans and great apes differed in any major way in "heritage" characters. In short, Gregory concluded, the differences among chimpanzees, gorillas and humans were what one would expect according to the "well-established principle of adaptive radiation," which of course was a term coined by Osborn himself.[167]

In regard to Osborn's argument that putative ancestors must possess the "potentiality" to produce the morphological characteristics of their descendants, Gregory conceded that this was true. But what exactly did it mean? Surely not that an ancestor must actually possess physical rudiments of all these characters. The paleontological record abounded in obvious exceptions to this notion, which Gregory called "a sort of emboitment hypothesis in which the visible characters of later forms are mentally imputed even to their very remote, undifferentiated ancestors."[168] If invisible, which it thus often had to be, "potentiality" was not a very reliable guide to the hypothetical reconstruction of fossils or the phylogenetic analysis of existing ones.

The final support of Osborn's theory was the "law" of "irreversibility" of evolution, and to Gregory, his mentor's interpretation of that principle was far too restrictive. Evolution was indeed "irrevocable" -- complex structures once lost could not re-evolve in the same form, nor could the many marks of an animal's "heritage" be erased but rather they had to form the base on which further change would build. In these senses "irreversibility" was true, but it did not rule out changes in the direction of evolution based upon changes in function. Applying this distinction to hominoid evolution, Gregory disputed the notion that brachiation had brought specializations -- "hook-like" hands with reduced thumbs, excessively long arms, and shortened legs were the ones Osborn had singled out -- that could not be reversed. Perhaps the modern great apes had become "over-specialized" for arboreal life, for they apparently had a few specialized characters such as the reduction of certain muscles in the hand that were irrevocable. Yet there was no evidence that what Gregory called a "brachiating pro-anthropoid" ancestral to both modern great apes and humans had to have possessed these specializations. In fact, the numerous characters which humans seemed to share only with chimpanzees and gorillas far outweighed these extreme brachiating specializations that could not be reversed. A recent common ancestor was still the most parsimonious hypothesis.[169]

Not only was there little support either in the data or in principle for ruling a relatively unspecialized brachiator out as a human ancestor, but Gregory could also point to mechanisms by which the necessary "transformation with change of function" had come about. Ironically, the one he chose to emphasize was the type of change in skeletal and dental proportion that Osborn had called an "allometron." In his most extensive treatment of "allometrons" -- contained in a massive monograph on the titanotheres, an extinct mammalian group -- Osborn had insisted that within a particular evolutionary line allometric change would always proceed in one direction only;[170] Gregory, however, opposed this view, arguing instead that evolutionary changes in proportion were not so constrained, and could take a new direction if such a shift served adaptive needs.

In the case of hominid evolution the major such need was for more efficient ground-dwelling adaptations during the Miocene radiation of the dryopithecine group. Nearly all the familiar differences between humans and African great apes -- relatively long legs and short arms, expansion of the brain case, retraction of the "muzzle," lengthening of the thumbs vs. the other digits, broadening of the pelvis, etc. -- were (as Gregory had long since pointed out) changes in proportion directly adaptive for terrestrial bipedalism and an omnivorous diet. That most of these changes were opposite in direction to the probable evolutionary trends leading to modern "highly specialized" brachiators did not prevent their having begun from the same "pro-anthropoid" starting point.[171]

In addition to identifying many of the "transformations" involved in human emergence as instances of "allometric" change, Gregory was also able to account for the interrelationships among those in the brain and skull by invoking the well-known principle of "fetalization" or "paedomorphosis." Though its principal proponent was the Dutch scientist Ludwig Bolk (1866-1930), Keith, Davidson Black and others, Gregory noted, had also pointed out "the far-reaching effects of the progressive retardation of the period of maturity in the progressive human line." The stretching out of ontogeny had allowed the hominids to retain, in "over-emphasized form," characters present in fetal apes, which in the normal, more accelerated course of anthropoid ontogeny were soon transformed.[172] For Gregory, the importance of the fetalization hypothesis was to be seen most in the skull and brain, for there it was obvious that the young ape -- with its proportionately large brain case, forward placement of the foramen magnum, small jaws and browridges, and faint muscle markings and supports -- approximated human characters in a manner fraught with evolutionary significance.[173]

All of these lines of argument clearly established the plausibility of the "habitus" transformation Gregory believed in. The only criticism of the "ape-man" theory left was one involving geological time. Osborn, like Keith, did not believe that a brain the size of Piltdown man's, or a skeleton as manlike as that of the Neanderthals, could have evolved since the mid-Miocene. Here again, though, Gregory seemed to have the better of his opponent. Since 1920 leading American geologists were beginning to estimate the lengths of various geological epochs using rates of decay of radioactive isotopes; these calculations were resulting in much longer durations for the Miocene and Pliocene than previously assumed. Now Gregory could reckon with a 15 million year span from the mid-Miocene to the beginning of the Pleistocene, a drastic change from the 1 million years he had discussed in

1920.[174] Not only was there plenty of time, but he could also point to the slightly "simian" characters that Boule, and more recently Morton, had supposedly discerned in the Neanderthal skeleton to imply that the transformation of "generalized dryopithecine" into _Homo sapiens_ was probably not fully complete even in the earlier part of the Ice Age.[175]

While Gregory's attack on the Osborn version of the "Dawn Man" theory was thorough and effective, there was another variant of considerable importance that he did not confront directly until after 1930. This was the "tarsioid" theory of human origins developed by Frederic Wood Jones. Wood Jones, a student of Elliot Smith, had first achieved prominence in the world of anthropology through his work on the "arboreal theory" of primate brain evolution.[176] Starting around 1920, Wood Jones began to use his findings in comparative anatomy to launch a new theory of human phylogeny -- one based on the notion that the human line had split off from the primate stem before the latter had given rise to either monkeys or great apes.

The most original species of data that Wood Jones depended on for this conclusion came from a close analysis of sutural patterns in the skull bones; the way these bones articulated with one another, he contended, differed in crucial ways in humans as compared with _either_ monkeys or apes. Surprisingly, though, the skull of the small, nocturnal Tarsius spectrum from the East Indies possessed a basic ground plan much closer to the human one. Since Tarsius was thought to have retained many of the basic characters of the tarsioid primates commonly believed ancestral to all simians, these correspondences proved to Wood Jones' satisfaction that the hominid line had split off directly from the tarsioid line during the Eocene; this was the reason why the human skull had been able to retain the "primitive" pattern. Thus, in his view as for Boule earlier and for Osborn, the many resemblances between large-bodied and large-brained forms like humans and gorillas were cases of convergence based on similar adaptations. They were much less important than fundamental patterns of cranial architecture, for as Wood Jones phrased it, in terms borrowed from Gregory himself -- "one fundamental structural difference begot of heritage outweighs many structural resemblances begot of habitus."[177]

Gregory had taken note of Wood Jones' presence in the ranks of "homunculus" theorists as early as 1920, but he provided no direct criticism of the latter's views until the debate with Osborn. Osborn had tried to make use of some of Wood Jones' data on the articulation of cranial bones to support his position. Gregory had replied with an argument that he was to develop in much greater detail later -- i.e. that Wood Jones' alleged "heritage" character was in reality a "habitus" character correlated with one of the primary elements in the human adaptive pattern -- the development of a large brain.[179]

Direct debate between the two writers occurred in the pages of the then recently founded periodical Human Biology in 1929 - 1930. Wood Jones had published a precis of his phylogenetic views and the evidence on which they were based; Gregory responded with a critique of that paper which focused on the way Wood Jones had used, or perhaps misused, the concepts of "habitus" and "heritage." "Habitus" and "heritage" characters, Gregory cautioned, did not segregate into neat piles as Wood Jones seemed to imply. What was "habitus" in an ancestor could be transmitted as "heritage" to a descendant; the fundamental condition to be met for both "heritage" to "heritage" and for "habitus" to "heritage" transmission was that no change of function and adaptation should have intervened to alter form. Character differences in the skulls of modern species, such as apes and humans, might denote different "heritage" but they might also have resulted from recent divergence in "habitus."

Most important, unless one were slavishly dedicated to allegedly universal principles like "irreversibility," one would often be hard put to decide whether a case of structural similarity between an "older" and "younger" form, such as humans and tarsioids, were a case of direct "heritage" or were itself a convergence due to "habitus" factors. All these problems could be solved only by analysis of a wide range of characters in many parts of the skeleton; a narrow examination of a few supposedly diagnostic characters could easily lead one astray.[180] If one undertook that broad-based analysis, he continued to maintain, the theory of an anthropoid "heritage" for humankind would still remain the most convincing.

Despite the great amount of time and energy that Gregory had devoted to the latter task in the years since 1916, he apparently thought that the message had to be delivered one more time. Thus in 1933, he took the opportunity to give a series of lectures at Oxford and the University of London to launch a restatement of the "ape-man" theory. The greatest part by far of these lectures, and of the small book that ensued, was devoted to a discussion of the evolution of the face and braincase; the importance that he attached to the need to refute Wood Jones' theories was evidenced by the amount of attention that he gave to this task along the way. Even the title of his book Man's Place among the Anthropoids, parodied Wood Jones' Man's Place among the Mammals (which in turn was a reference to T.H. Huxley's classic Man's Place in Nature).[181]

In line with his belief that phylogenetic relationships could not be traced on the basis of small samples of supposedly diagnostic characters, Gregory devoted the first part of Man's Place among the Anthropoids to a review of existing data on various parts of the primate postcranial skeleton. He went over the anatomy of the foot, femur, pelvis and sacrum, pectoral girdle, humerus and hand in order to demonstrate the same conclusion he had maintained against Osborn -- though differences clearly existed between pongids and humans, the unique structural similarities they shared were far too numerous to be attributed to "parallelism" or "homaeomorphy" (Wood Jones' equivalent of Osborn's "homoplasy" or "convergence"). Also, what differences did exist could be clearly related to "habitus" factors -- they were either differences in proportion in basically the same morphological pattern, or were progressive reductions of "ape-like" characters, the ultimate cause of both being the adoption of terrestrial bepedalism in humans.[182]

Though Gregory's discussion of the evolution of the face and skull was more involved, it resolved itself in the same fashion. Gregory described Wood Jones' data on the patterns of articulation of cranial bones, and the latter's theory about the differing systems of "cranial growth centers" in humans and anthropoids that had supposedly produced these patterns (as befitted Wood Jones' phylogenetic ideas the human type of cranial growth was billed as the more "primitive").[183] The American scientist praised his opponent's description of the anatomical facts as "scrupulously accurate," but also asserted that the theory explaining it, while "conscientious, consistently elaborated and ingenious" was "based logically on his [Wood Jones'] own peculiar postulates." Wood Jones' own illustrations of regions like the interior base of the brain case, Gregory contended, revealed that the differences described actually did little to obscure "the profound unity of plan in the skulls of man and ape that securely ties man to his cousins, the gorilla and the chimpanzee." "Of course," he continued,[184]

> there are differences in growth, that is, in the time and intensity of development of each part of the skull. If there were not, there would never have been either apes or men. But to assume that such differences in growth rates make it necessary to derive man from an unknown cousin of _Tarsius_ is, I submit, not supported by the facts so ably depicted in Professor Wood Jones' excellent diagrams.

To support his conclusions Gregory provided his own description, with illustrations, of this "unity of plan," and tried to account for the admitted differences by invoking "diversity of habitus." The essence of his case was the argument that each major difference in sutural patterns Wood Jones had identified could be correlated with the much greater size of the human brain, and the much smaller size of the human dental apparatus, relative to the overall dimensions of the skull. For Gregory it was easy to accept the notion that differences in the "time and intensity of development" of various skull bones coincident with these adaptive changes might alter sutural patterns somewhat. He saw no reason to enshrine the latter as unalterable marks of "heritage" rather than as plastic characters responsive to the function of cranial bones as coverings for the brain or zones of attachment for the muscles of mastication.[185]

As he had in his debates with Osborn, Gregory also invoked ontogenetic and neurological evidence that buttressed the findings based on osteology. Relying heavily on Frederick Tilney, he reviewed the fundamental homologies in the human and pongid brain stem, cerebellum and cerebral cortex; these homologies seemed to reveal not only that pongids approached the human pattern of brain organzation more closely than other animals, but also that no specializations had been found in apes that made the derivation of the human pattern from a generalized anthropoid improbable in the least.[186] Looking at the skulls of immature great apes, Gregory was able to illustrate in several cases a much closer approximation to human sutural patterns than was typical in adult apes. As well as giving credibility to his belief in "unity of structural plan" this data lent in his view "a certain amount of support" to Bolk's theory that paedomorphosis was a key factor in human evolution. That this theory had been "all but ignored by Professor Wood Jones" was also important; Gregory attributed it to the fact that the implications of Bolk's doctrine seemed to "run foul of the law of 'Irreversibility of Evolution.'"[187]

The evidence of comparative anatomy bearing on the "transformation" of a brachiating anthropoid into an ancestral hominid thus seemed stronger than ever. In addition Gregory had reason to believe that more was known about the possible mechanisms involved. He put great store in Keith's notion that slight differences in endocrine function might have had far reaching effects on both anthropoid and human morphology, since the secretions from these glands were "known to influence skull form, growth, limb proportions, and hair characters."[188]

Modern genetic theory seemed to make the idea of relatively rapid morphological change easier to accept as well. Several ape-human character differences might, for example, have been non-adaptive "resultants of such new 'cross-overs' or other irregular combinations of genes as may have had no special value but were not injurious."[189] And whether one was dealing with adaptive or non-adaptive characters, one lesson of genetic theory seemed particularly relevant -- i.e. that there were[190]

> so many ways in which both qualitative and quantitative characters may be altered by hybridism, segregation and inbreeding that it seems anachronistic to attribute to the very remote Tertiary ancestors of man the long legs, long thumb, big brain, short face, small canines, etc., which are now diagnostic human characters.

The ultimate source of Gregory's confidence in the "ape-man" theory of human evolution, at least as he treated it in Man's Place among the Anthropoids, was in comparative anatomy. Still, he believed that the paleontological data was on his side as well, and that it had shifted even more in this direction in the ten years previous to 1934. The range of the dryopithecine group had been extended to East Africa by British scientists recently, for example. While this provided better evidence that the group to which Gregory looked for man's anthropoid ancestor was a widely distributed one, there was as yet "no evidence in favor of Wood Jones' theory that during the scores of millions of years between the lower Eocene and the lower Pleistocene there existed a wholly unknown phylum of primates" ancestral to humankind.[191]

There was also support from what Gregory considered the "two greatest" fossil finds relating to human evolution of the last decade -- i.e. Australopithecus and Peking man. Since Australopithecus and its relationships assumed a very prominent place in Gregory's research in paleoanthropology during the 1930s, what Gregory said about the Taungs fossil in 1934 will be discussed in more detail below.[192] At this juncture all that needs to be pointed out is that Gregory portrayed Australopithecus as an ideal "structural ancestor" for various characters in later hominid forms, and as a creature that revealed in several ways how the transition from anthropoid to human could have come about.

Gregory also found "Sinanthropus pekinensis" relatively easy to fit into his conception of hominid evolution. The analysis of the "Sinanthropus" skull and dentition by Davidson Black and Elliot Smith had, he noted, made "its primitive human characters incontestably clear," and Teilhard de Chardin had produced "abundant evidence" for dating the fossils as lower Pleistocene. Calling the skull "a little more advanced" in overall appearance than "Pithecanthropus," Gregory also pointed to characters like the form of the tympanic bone and the general shape of the occiput that were reminiscent of conditions encountered in anthropoids. Though its brain case was, he said, "remarkably small ... all the lower teeth were advanced towards the human stage."[193] These conditions made it appear "obvious" to him that the "Sinanthropus" skull represented "an early stage in the lengthening out of the process of development, as suggested by Davidson Black," who was also a supporter of the paedomorphosis theory of Bolk.

Finally, as interpreted by Black and Elliot Smith, "Sinanthropus" seemed to possess a small and quite primitive endocranial cast. This "fact" seemed to throw great light on the much vexed question of the rate of hominid brain evolution. For if one plotted a curve based upon the rapid increase in cranial capacity from the lower Pleistocene "Sinanthropus'" 900-960 cc. to modern man's 1200-1500 cc. and extrapolated it backward, one would come down to the "600 cc. upper limit of the anthropoid brain at no distant date of the Tertiary."[194] A mid-Miocene appearance of the first true hominids thus looked better than ever, if of course one accepted the unspoken assumptions here that the rate of hominid brain evolution was uniform, and more importantly, that "Sinanthropus" was either a direct human ancestor or a not too "conservative" cousin. At that time, however, writers like MacCurdy and Hooton were rejecting the latter contention.

Even though they contained a large element of hypothesis, these observations on the evolution of the brain provided a fitting climax to Gregory's argument. If this most human of characters could have evolved from those possessed by a "lowly" brachiating ape during the latter half of the Tertiary, then what character could not? Despite what the "Dawn man" theorists had claimed, for him evidence from "many lines of investigation" converged upon the same conclusion -- the best theory of human origins was still what he considered to be the orthodox one. When examined as a whole, the record continued to provide "abundant confirmation of Darwin's general position" about "the relative nearness of man to the anthropoid apes in the system of nature."[195]

The Theoretical Context of Primate "Transformations"

From the perspective of the present day, when chimpanzees are the consensus choice for humankind's closest living relative, and fossils like Australopithecus afarensis seem to suggest that key parts of the human adaptive pattern have appeared only recently on the geological scene, it is easy to give a "Whig" interpretation to Gregory's work on primate evolution.[196] From this point of view Gregory could be said to have argued as he did because he had a better grasp of the data as a whole than did his opponents, whose understanding was clouded by outmoded theories and the habit of hypothesizing hominid ancestors more "human" than any of the existing fossils. While this idea has merit in relation to Osborn's attack on the "ape-man," and the unwillingness of MacCurdy, Hooton, and to a certain extent Hrdlicka, to give sufficient weight to Australopithecus and Peking man, we still must exercise caution. For one thing, Gregory's ideas on later "types" of fossil hominid were much more conventional and less critical of accepted conclusions than his work on hominid-pongid relationships. After the early 1920s he seemed to make little effort to question Piltdown, or the relegation of Pleistocene "low-brows" like the Neanderthals to side branches of the human family tree, and as Hrdlicka had shown, grounds for criticism were not hard to find.

In addition, Gregory's views on the anthropoid heritage of humankind were not being so roundly attacked as the intensity of his defense would indicate. Hrdlicka, Hooton and MacCurdy all accepted the importance of the dryopithecines in human evolution, after all. Comparative anatomists like Schultz and Morton had reservations about large-bodied "brachiating" ancestors, but they never tried to call into question the close relationship of humans with other hominoids.[197] These writers might see an Oligocene "gibbonid" rather than a Miocene pongid as the last ape-human common ancestor, but this was not a rejection of "ape-men" per se.

Thus, explanations other than just superior scientific judgement need to be invoked to account for Gregory's persistence in defending "ape-men," and later "man-apes" (i.e. the australopithecines). There were probably personal factors involved -- e.g. a need to "defend a territory" once he had staked it out with his work on the "Y5" pattern; while his obvious target between 1925 and 1934 had been "homunculus" theorists, the defense of a dryopithecine ancestry for humans was also a constant, and very important objective. In addition, there was clearly a friendly, yet serious kind of rivalry with his mentor Osborn. More purely intellectual motives seem to have seen involved as well, though. Part of Gregory's fascination with the "ape-man" and "man-ape" problems seemed to reflect the fact that they were special cases of a phenomenon whose workings he tried to trace in all of his studies of vertebrate anatomy and phylogeny -- evolutionary "transformation" attendant on change of function and adaptation.

We have already seen this interest in transformation displayed in Gregory's debates with Osborn and Wood Jones and his discussion of "habitus" and "heritage," but it is visible as well in writings on the evolution of specific structures in vertebrates. Most relevant to the problem of human evolution were various works on the vertebrate skull and face. The first of these appeared prior to 1920 and set a pattern for later efforts. In them Gregory laid out a series of structural "stages" in the progressive development of the face from the lobe-finned fishes of the Devonian Age to modern humans, and analyzed the key transitions that had occurred in each stage, such as change in proportions of homologous structures, fusion of once distinct structures into one, and reduction or loss of primitive structures in more advanced forms.[198] The list of such "stages" originally numbered eight, but from 1927 onward the usual number became ten.[199] Both in the earlier and later writings Gregory used both fossil and recent species to illustrate "stages;" he was not aiming to establish a strict line of ancestors and descendants, but rather to delineate what he came to call in the late 1920s "basic patents" -- i.e. the critical constellations of characters that represented new departures in form and adaptation, and that provided the base upon which human patterns would eventually be erected.[200]

Gregory once noted that he began to use the word "patents" to describe these character complexes for three reasons. First, like human inventions the structures involved could often be seen to employ basic mechanical principles such as the lever. Also, they could be conceived as embodying "the results of a long line of trial and error," and were "subject to the guiding force of selection operating in a given direction" in a way similar to human technology. Finally, there was an "anticipatory" element in them that in his view mimicked the behavior of human planners. Biological structures had a way of developing before they were used, as if heredity had somehow preserved a memory of the environmental problems encountered by past generations.[201]

Just as important as these explicit reasons for chosing the term "patents" was the implicit rationale for identifying some of them as "basic." In all of his writings on the heritage of the human skull, pelvis, etc. Gregory tried to focus on the points where evolution underwent a reorientation, where new structural patterns and new modes of adaptation could be seen emerging from older ones. The theoretical significance of such reorientations was clear to Gregory but he believed strongly that it had not been sufficiently noted by quite a few students of evolution, and particularly by those who could not abide "ape-men" as human ancestors. The latter, he argued in an address before the American Association of Physical Anthropologists in 1935, had tried to make principles like "orthogenesis" and "irreversibility" the master keys to the evolutionary process, while in reality they could only unlock part of the mystery.

To be sure, Gregory admitted, "undeviating evolution," in which "characteristics that are already observable in a moderate degree in a remote ancestor become more and more accented in the descendants" was amply documented in vertebrate evolution.[202] Examples in the case of the primates would be the "enormous expansion of the braincase and the pushing up of the forehead into a nearly vertical wall by the forward growth" of the brain, along with their neurological accompaniment -- "the transition from paleokinesis, with its more immediate responses to sensory stimuli, to neokinesis, which in the later stages ... makes possible the control of conduct by ideas." These processes could indeed be followed backward though the primate line, from humans to forms antedating the dryopithecines.[203]

Just as crucial, he insisted, as "undeviating" directional changes, however, were "transformations" --

i.e. cases where there had been reorientations "in the life medium and anatomical habitus, so that both ordinal or family habitus and class heritage are widely different and the descendant bears but little resemblance to its more remote ancestors." Transformation could involve the reduction or total disappearance of older structures, the imposition of new structures over ancestral "plans," or changes in proportion that reversed the direction of previous evolutionary paths.[204] Again he claimed that human evolution provided numerous examples, and specifically cited the changes in the foot, pelvis, and facial superstructure that he had used to good effect in the debates with Osborn.[205]

Thus, for Gregory evolution was much more flexible, and much less predictable, than it was for the proponents of orthogenesis and irreversibility whom he criticized. "Undeviating evolution" and "transformation" were complementary processes; together they described a world in which organisms could respond slowly and uniformly to stable or slowly changing environmental conditions, or rapidly, and perhaps radically, to new adaptive requirements or opportunities. The actual data and specific inferences about human evolution that he was transmitting to his anthropological audience were not new. Rather, Gregory was trying to make them sensitive to the broad theoretical implications of phylogenetic relationships and adaptive patterns that he had long sinced traced to his own satisfaction.

Though it must be a tentative exercise given the relative lack of data that has been compiled about Gregory's personal and social attitudes, it is possible to speculate about why he would so vigorously promote this more open-ended conception of evolution. If Osborn's later theorizing was so closely correlated with his pride in family and class, with his sense that success in life was the playing out of potential clearly present in those blessed with good breeding, Gregory's ideas paralleled his own social course from the apartment above the family printshop to the laboratories and lecture halls of a great university. In Gregory's world "ape-men" could become "true men" (the revealing term used by French anthropologists to describe anatomically modern humans) given the right environmental opportunities and pressures; in Osborn's "little men" grew up to be "big men" in the same world in which their fathers were raised.

Whatever the underlying motivations for some of the ideas he put forward, the fact remains that in the mid-1930s Gregory seemed to take great interest in speculating about evolutionary principles generally. In addition to those noted above, he also wrote about another pair of complementary principles in vertebrate evolution, which he called "polyisomerism" and "anisomerism." Polyisomerism he defined as "the state in which many homologous parts, or polyisomeres, are arranged along a primary or secondary axis, whether straight or curved;" anisomerism was "the state in which one or more parts [of an organic structure] are emphasized at the expense of the rest, while the original number of separate parts is usually reduced, either by fusion or elimination."[206] Polyisomerism he considered to result from a growth process analogous to "budding" in invertebrates, while anisomerism resulted from unequal growth or omission of parts in a polyisomerous series. Since either or both processes could operate at any stage in a vertebrate evolutionary line, dramatic structural transformation seemed to be a hallmark of vertebrate evolution, especially in the skull and dentition. When for example one analyzed the series of stages or "basic patents" in the line leading from early vertebrates to humans, one could, he thought, see anisomerism at work particularly well in the evolution of the skull, where reduction in the number of separate bones and "allometric" changes in their relative dimensions played a major role in determining the shape of the mandible and skull vault.[207]

Gregory's reference to "allometry" in this discussion implicitly acknowledged the fact that his own search for general evolutionary principles owed a good deal to Osborn's efforts in that line. Indeed, in his analysis of polyisomerism and anisomerism he took pains to show how Osborn's general categories describing evolutionary change in the vertebrate skeleton -- "allometrons," "rectigradations" and "aristogenes" could be subsumed under his own principles.[208] Like Osborn's principles, too, Gregory's were descriptive rather than explanatory; in the articles he devoted to these concepts there was no discussion of the genetic or developmental mechanisms that produced the various forms of structural change, or of why one form occurred in a given situation. In fact, the closest he came to a theoretical discussion of how skeletal evolution came about was not in an article about general principles or a discussion of human emergence, but rather in his important monograph on fish skulls, which appeared in 1933.[209]

Even though Gregory's book did not deal with human evolution directly, a brief look at its theoretical passages is important if we are to understand his concept of the evolutionary process. In this work, which he called a study in "growth and form" as well as an analysis of the skull types found in various kinds of fish, he tried to show how in teleost fish (the group possessing bony rather than cartilaginous skeletons) a "basic patent" of bone, cartilage, and muscle persisted beneath a myriad of outwardly distinctive forms. Gregory also tried to demonstrate how modifications of the basic skull plan constituted mechanically successful adaptations for specific locomotor and feeding habits. As in the case of the primate skull, Gregory argued that the most common mechanism for producing these adaptations was differential growth of homologous structures, or "anisomerism."[210] Where Gregory went beyond the ideas contained in his writings on the primates was in his clear attribution of the cause of these adaptive shifts to the process of natural selection -- which he believed that he defined "as Darwin did, as a sort of personification of the vast complex of active forces and passive conditions which cumulatively result in hereditary differences between descendants and ancestors."[211]

While natural selection was the ultimate cause of change, the proximate causes, Gregory theorized, were changes in the "mechanism of regulation," which operated by means of the endocrine secretions and the so-called chemical "organizers" that apparently controlled the various growth centers in the developing embryo.[212] He also conjectured that the need for correlated changes which ensured harmonious functioning in new adaptive complexes favored a process of evolution by selection of what he termed "genic systems" -- genetic patterns that could produce change in several structures at once via the growth process. He even ventured the guess that perhaps these new "systems" could arise in a single generation. Whether these speculations made genetic sense -- Gregory was apparently trying to describe a selectionist version of Mendelism that would still allow for the type of abrupt, dramatic change usually associated in his day with DeVriesian macromutations -- or not, they serve to undescore his "transformationism" quite strongly.[213] Even more powerfully than in his writings on primate evolution he was pointing out how rapidly major adaptive changes could come about, and that there were genetic and physiological mechanisms common to all vertebrates that could bring such changes to pass.

In the monograph on fish skulls Gregory also discussed the uses to which his evolutionary principles could be put in the task of reconstructing phylogeny, and he did so in a way that illuminated some of his ideas about primate evolution. Thus he argued that the functional approach he was employing could promote a "synthetic" instead of the normal "analytic" style of taxonomy. That is, he believed that looking for underlying similarities in form rather than focusing primarily on small differences would produce useful phylogenetic generalizations, a much needed corrective to the habit of "particularism" that seemed to afflict phylogenetic discussion at the time.[214]

As noted earlier, Gregory had also made a similar complaint against the opponents of the "ape-man" theory -- that they had allowed small structural differences in supposedly diagnostic characters to rule out close kinship between pongids and hominids. The use of such arguments by Osborn and Wood Jones was thus an instance of what Gregory conceived as a more general phenomenon, and was not exclusively associated with writers who believed in Eocene "homunculi" or rigid principles like "orthogenesis." Functional explanations of minor character differences, which could then be explained as adaptive responses, were all too often ignored, he felt, by those who followed the "analytic" or "particularistic" style of taxonomy. It was thus no surprise to Gregory to find that questions continued to be raised about the degree of relationship between pongids and hominids even by comparative anatomists who were not enamored of the various "Dawn man" theories.

An interesting example of the kind of reasoning that Gregory rejected as "particularistic" was the analysis that a younger American scientist, W.L. Straus, Jr. had done on the primate pelvis in the late 1920s. Straus, after examining the form of the pelvic bones in a sample of humans, great apes and Old World monkeys, had identified several characters in which humans apparently resembled monkeys more closely than they did apes. Straus had then gone on to use these differences to argue against the theory that humans were more closely related to great apes, since they would have lost these "primitive" characters if they had passed through a pongid stage. Though Gregory accepted Straus' facts as accurate, he was critical of the conclusion, for he thought that all of the differences between humans and apes that Straus had noted could without great difficulty be interpreted not as "primitive" inheritances but as new conditions related to the attainment of upright posture. In Gregory's view, transformation of a pongid pelvis to accomodate this change in function had produced a few individual characters <u>similar</u> to those in monkeys, but they provided no evidence of direct descent.[215]

It would, of course, be easy to argue that this type of difference in interpretation did not at all carry the theoretical significance that Gregory gave it -- perhaps Straus merely saw his man-monkey resemblances as true homologies, while Gregory counted them as parallelisms, and each man felt in an opposite way about Gregory's man-ape resemblances. Perhaps such judgements are subjective enough that scientists who follow the same basic methods can and will differ when making them. Nevertheless, Gregory's differences with Wood Jones show clearly that when and how often one invokes concepts like homology, parallelism and convergence can depend on hypotheses about function and adaptation. Just as important, those who consciously seek out functional explanations probably do tend to rank their various pieces of evidence in very different ways from those who try to count up similarities and differences in a more strictly empirical fashion. Indeed, without the luxury of a very full fossil record, it would be difficult to make judgements about "primitive" versus "derived" characters in the absence of hypotheses about adaptation and function, since one would have no criterion for being certain that many supposedly "primitive" characters possessed in common by an "older" and a "younger" speices were not cases of convergence or parallelism.[216]

Gregory may have been right that "particularistic" analysis of skeletal characters might tend to overlook crucial evidence of phylogenetic relationships, but there were pitfalls in his own approach as well. If the hallmark of the particularistic style, with its emphasis on small character differences, was the multiplication of species and genera, the reverse problem afflicted the search for "basic patents" and "plans" based upon adaptation. Once these "patents" had been identified, the differences that might exclude creatures from actual ancestor-descendant relationships with each other could easily be glossed over. One can illustrate this point from Gregory's own descriptions of "stages" in the evolution of various human skeletal and dental patterns; Gregory used living creatures, like the female chimpanzee, to represent some stages, while for others he picked fossils like the so-called "Mousterian youth" whose proximity to the line leading to modern humans he was not certain about. Though he did not try to assert that these "stand-ins" were identical to human ancestors in the characters under discussion, he was implying that significant differences were not likely. Because nature may have more than one way to solve a particular adaptive problem or may take diverse paths to achieve a certain result, it was a risky business to "preempt" paleontology to hypothesizing about true ancestors on the basis of conditions found in past and present collaterals.[217]

The "interchangeability" of closely related forms in Gregory's reconstructions of "stages" in human evolution points to another difficulty with his version of functional analysis -- i.e. a certain lack of rigor in inferring function from anatomical detail. Though as far back as 1912 he had tried to describe the locomotor styles of various groups of ungulates by applying mechanical principles derived from basic physics, in his later writings on "basic patents" there is little that can be considered biomechanical analysis. For example, in analyzing the characters relating to locomotion he seemed to reason as follows: we know that the pelvis, foot, etc. of bipedal humans and quadrupedal "brachiators" differ in the following ways, therefore the differences must all be part of each group's adaptive pattern. How the part actually worked would be described only in the most general way and did not result from any detailed study of either living creatures in motion or from that of abstract models derived from mechanics. Thus Gregory interpreted the evidence provided by his wide ranging studies in comparative anatomy and paleontology in the classical descriptive manner, and did not consciously try to generate testable hypotheses from it. While his work was "dynamic" because of its emphasis on functional and adaptive interrelationships among anatomical characters, it was "static" in the way it produced and analyzed the data.

We should not, however, judge Gregory's approach by the degree to which it did, or did not, anticipate modern approaches to similar questions, for it had great strengths when compared to those adopted by the writers he was criticizing. First, it made the most of existing fossils and did not invent hypothetical ancestors different from any fossils yet discovered. In addition, Gregory's hypotheses about shifts in adaptation and accompanying structural changes, though based in descriptive morphology almost exclusively, presented a scenario of higher primate evolution that was a fruitful point of departure for further analysis. Questions such as how diet and locomotor adaptation were related in early hominids, an issue that Gregory first confronted in 1916, are still prominent in considerations of human evolution.[218] Most important, Gregory's application of general evolutionary principles to humans, and especially his stress on the key principle of transformation, tied humans securely into the structure of vertebrate evolution and constituted a valuable counterpoise to the often unconscious habit of treating humankind as a special case in evolution. Though it is true that Gregory himself did not fully avoid this habit in his handling of later "races" of fossil hominid, his vigorous and largely successful attack on what he eventually came to call "pithecophobia" helped immensely in demystifying the problem of human origins.

Adding the Australopithecines to the Human Family Tree

Despite the strength of theoretical arguments and the abundant evidence for them provided by comparative anatomy, the real test of an evolutionary scenario has to be its conformity with the fossil record. For Gregory specifically, the great need was for fossils of pre-Pleistocene age. In 1925 a new fossil primate was reported which would eventually cast a great deal of light on the phases of human evolution in which Gregory was most interested -- namely *Australopithecus africanus.* But illumination did not come instantaneously, and it was not of the sort that sat comfortably in the eyes of all viewers. Gregory, however, would become *Australopithecus'* principal American defender at a time when Hrdlicka and Hooton, for example, were still cautiously characterizing it as an "interesting" fossil anthropoid.[219]

At first, however, Gregory was more in line with the prevailing, cautious evaluations of the Taungs fossil. In an explicitly "tentative" phylogenetic tree that he drew in 1927, for example, he put *Australopithecus* squarely into the chimpanzee-gorilla group of African hominoids, and closer to the former than the latter genus, since he sketched a Late Miocene divergence between

Australopithecus and the chimpanzee, and a mid-Miocene split betwen the latter and the gorilla.[220] Gregory also tried to use an "ape-like" characterization of *Australopithecus africanus* as a debating point against the "pro-Dawn Man" of Osborn, when in 1929 he observed that the discovery of an "anthropoid" fossil such as the Taungs specimen in the arid plains country of South Africa disproved Osborn's views about the diametrical opposition between the "plains-living" psychology of the "pro-Dawn Man" and the "forest-living" psychology of the anthropoid line.[221]

As early as 1930, though, Gregory's ideas on the australopithecines had taken a new direction. On the basis of photographs of the dentition of the Taungs skull sent to him by Raymond Dart, he was willing to defend Dart's claim that there were hominid-like characters in the australopithecine dentition. The conclusions that emerged from the analysis of these photographs were as follows: the Taungs fossil's first lower molar was a permanent tooth which appeared to possess "the complete *Dryopithecus* pattern," but it also had the "sixth cusp so often found in man," as well as a relative breadth which seemed "more human than in any fossil or recent anthropoid" he had seen. The first lower premolar, a deciduous tooth, was said to possess the "submolariform" shape common in humans and was not "compressed and premolariform" as in the larger pongids. Noting that the upper dental arch was also very suggestive of the "primitive human" shape, Gregory then produced a summary table which claimed that, out of 26 dental characters of Austrolopithecus analyzed by Milo Hellman and himself, fully 20 represented conditions "transitional, or nearer to, primitive man" while only 3 were nearer to the chimpanzee and/or gorilla. This dental evidence appeared so powerful that Gregory wondered rhetorically, "if Australopithecus is not literally a missing link between an older dryopithecoid group and primitive man, what conceivable combination of ape and human characters would ever be admitted as such?"[222]

As if to underline the fact that he was not using words like "missing link" for mere effect, Gregory tried to fit Australopithecus into his by now standard scenario of human emergence. While for a long period, he asserted, nature had seemed to be "bent on breeding better and better brachiators," it eventually had[223]

> segregated some of the more conservative brachiators, turned them out of their forest home and started their evolution in a new direction, that of upright walking upon hard ground. Australopithecus, to judge from its skull and dental characters, was a pioneer in the new line, as held from the first by Dart.

Australopithecus could also be made to support the

American's view of the timing of hominid emergence. Though it was not securely dated, most scientists seemed to agree that the Taungs fossil was not ancient, probably no earlier than the "late Tertiary." If the fact that the fossil combined "primitive human" dental characters and a "progressive anthropoid" brain indicated, as Dart believed, that Australopithecus was not far from the "common source" of the hominids and African apes, then Gregory's hypothesis of a relatively recent, Miocene divergence between these families would be strengthened.[224]

The great importance that Gregory was beginning to place upon the australopithecine group came through clearly in an interview that he gave to a writer for the Popular Science Monthly in 1931. There, in response to the perennial question about the "missing link," he replied that "if by 'missing link' you mean a specimen that seems to bridge the gap between the highest ape forms and the most primitive humans, then, in my opinion, the little South African fossil man-ape comes closest to filling the bill." Giving a brief summary of the tooth and skull characters that made this diagnosis possible, he added that the location of Taungs in a region that had long been "semi-desert" gave additional strength to his conclusion, since it was in just such places "far away from any forest, that scientists look for the birthplace of humanity." Though there was still debate over the exact location in the Old World where the transition occurred (central Asia was obviously in his mind here) and over the age of Australopithecus, Gregory nevertheless felt "confident that it was this kind of creature that heralded the advent of man."[225]

By 1934, Gregory was also pointing out that Australopithecus' morphology harmonized with two other theories that figured prominently in his account of human evolution -- "fetalization" and "neokinesis." As Dart interpreted the Taungs skull, it not only had a shorter and less robust facial structure and a more rounded forehead than a chimpanzee of similar "dental age," but also a more advanced brain according to its endocranial cast. These conditions made Australopithecus a perfect replacement for the female chimpanzee that Gregory often used to represent the "progressive dryopithecine" point in hominid emergence. While he conceded that the Taungs specimen's "muzzle" was "shorter than that of the adult of the same race," this fact merely served to lend color to the notion that humans were in a sense "infantalized anthropoids." "In any case," he asserted,[226]

> we no longer have to invent for man hypothetical ancestors with short faces and reduced canine teeth when this late Tertiary ape gives us a real skull of this kind. The peculiarly human characters of the

> skull probably developed [at a later evolutionary stage] _pari passu_ with the power of speech and the great expansion of association systems in the brain.

In this case theory and an important discovery magnified one another. With Taungs to suport him, Gregory could be more confident than ever of his evolutionary scenario, but it is also clear that the hominid status of _Australopithecus_ would seem much more convincing to one who already believed in the geologically recent transformation of large-bodied apes into hominids.

While Gregory was definitely _Australopithecus'_ strongest American proponent in the early 1930s, he still held back from giving the fossil a definite phylogenetic status in the human family tree. Respect for majority opinion may have played a part here, but the variety of extant fossil human remains and the controversy surrounding several of them also made him cautious. As he put it in 1931,[227]

> we now have so many different kinds of fossil men -- that is, fossilized remains of pre-human types -- that it is hard to determine their relationships to each other and to their ancestors ... their present number is so large as to be confusing, but not sufficiently large to settle the question.

Making conclusions about phylogeny on the basis of a single skull was risky; in Gregory's specific case the need to eat crow concerning the alleged fossil anthropoid "Hesperopithecus" had provided excellent warning over the need for care.[228] In addition the immaturity of the Taungs fossil was an often cited reason for postponing taxonomic judgements.[229]

Further discoveries, and especially of adult specimens, were necessary if more was to be made of the australopithecines. And in fact new fossils did come to light in the second half of the 1930s; this time they came not from Taungs but from the sites of Sterkfontein and Kromdraai, while the scientist who was instrumental in describing them and promoting their significance was not Dart but rather Robert Broom (1866-1951), the South African vertebrate paleontologist. As did Gregory, Broom attended the important Conference on Early Man that was held by the Philadelphia Academy of Natural Sciences in 1937. He brought along casts of the first Sterkfontein fossil, which in view of its similarities to the Taungs specimen Broom had initially named <u>Australopithecus transvaalensis,</u> but later put into a new genus, "Plesianthropus."[230]

Gregory was understandably eager to examine this specimen and the others assembled by Broom in South Africa first hand. In March, 1938 Broom invited him to come to the Transvaal Museum to study the australopithecine dentition. Soon after this Dart extended Gregory an invitation to examine the Taungs fossil was well. In the summer of 1938 Gregory and his longtime collaborator in the study of higher primate dentition, Milo Hellman, set out for South Africa.[231]

The material that Gregory and Hellman had access to was impressive. By mid-1938 at least partial dentitions had been collected from several individual "man-apes." In the monograph on their findings that was published by the Transvaal Museum, they catalogued the specimens they had studied as follows:

> *Australopithecus africanus* Dart. One superbly preserved skull with dentition, including nearly the entire deciduous set and the first permanent molars of the upper and lower jaws.
>
> *Plesianthropus transvaalensis* Broom. Upper grinding teeth (p ,p ,m ,m ,m) of male type. Upper lateral incisor, canine, p ,m , of referred female maxilla. Lower canine,p and m , referred specimens. Considerable data referring to alveoli and roots of teeth.
>
> *Paranthropus robustus* Broom. One adult palate with well-preserved molars and premolars. One right half of mandible with well-preserved molars and premolars.

Additionally there were endocranial casts from both *Australopithecus africanus* and "Plesianthropus" along with assorted skull fragments that could "afford an additional check" on conclusions arising from the dental evidence.[232] The time for making hypotheses hesitantly seemed to be at an end -- Gregory and Hellman expressed their confidence that "at least according to the experience of paleontologists, the material listed should be adequate for determination as to its systematic position among the higher primates."[233]

In their attempt to assess the "systematic position" of the australopithecines, Gregory and Hellman undertook a detailed morphological analysis of the dentition similar to the one they had done on the dryopithecines in 1926. They included both minute description of crown patterns and calculations of tooth dimensions and metrical indices, and compiled observations from modern and fossil pongids and hominids for purposes of comparison. Their choice of fossil species for analysis seems to have been dictated by relative abundance of material -- most of the detailed comparisons were made between the australopithecines and either Sivapithecus specimens from the Siwaliks or the "Sinanthropus" dentition that had just been described by Franz Weidenreich.[234] Not surprisingly, since they considered themselves to be individuals not "thoroughly conditioned in the ritual of the statisticians" the samples they used were small. For example, the metrical indices they calculated for the upper premolars of gorillas, chimpanzees, "Sinanthropi," orangs and modern humans were based on pairs of individuals from each species. The measurements for the molar teeth of chimpanzees and gorillas reflected much larger samples but this was possible because the authors relied upon statistics published earlier by the German scientist Adolf Remane.[235]

In part these small samples were unavoidable results of scanty fossil evidence, but they also reflected the belief of the authors about the relative value of metrical and non-metrical evidence. As they confessed at one point in their monograph, Gregory and Hellman were "convinced from long experience that, in the assessment of degrees of phylogenetic relationship between several related forms, breadth indices of tooth crowns" often showed wide individual variations and were thus "of much lower value" as indicators than crown patterns.[236] This is not to say that they tried to ignore metrical data, but rather that for them evidence from crown patterns carried the main burden of proof when it was available.

The results of the Americans' analysis of individual tooth types -- incisors, canines, premolars and molars -- all pointed in the same direction, though to different degrees: the australopithecines were intermediate between dryopithecines and primitive humans like "Sinanthropus." Thus the australopithecines' upper central incisors were said to resemble an incipient version of the "shovel-shaped" incisors visible in what they called the "*Sinanthropus* - Mongoloid series" of later hominids.[237] Similarly, they concluded that a "readjustment in growth rates" in each portion of the upper premolar had produced a transitional form in which the "buccal asymmetry" characteristic of dryopithecines was reduced and a more hominid-like "oval-shaped contour" with buccal and lingual cusps more equal in size had resulted.[238]

More important still was the evidence provided by the canine teeth. The "female Plesianthropus," they said, had an upper canine that was "more simplified in the human direction than the 'premolariform' canine of the female Sivapithecus," while the "size and position of the roots" of the "male Plesianthropus" indicated a far less robust tooth than the "enlarged and tusklike" canines found in Sivapithecus males.[239] Also, the lower canines of both sexes in "Plesianthropus" were smaller than in Sivapithecus or in modern pongids.

Finally, Gregory and Hellman found that the occlusal pattern of the australopithecine canines was distinctly atypical of apes, for there was no "diastema" or gap in the tooth row between the upper canine and lateral incisor. While the authors admitted that the "diastema" was not a universal character in pongids, there did not seem to them to be "space enough" between the lateral incisor and canine margins for "the accomodation of the lower canine tip." In sum, the size and shape of the canines as well as this indication of an "edge to edge bite" in the front teeth seemed to suggest "primitive hominid conditions" and not just an intermediate position between pongids and hominids.[240]

For the discoverer of the "Dryopithecus pattern" the form of the molar teeth was almost sure to carry the greatest weight, and here too the diagnosis of "primitive hominid" seemed appropriate. Australopithecine upper molars showed cusp patterns which could be matched closely by individuals in the "Sinanthropus" and even the Neanderthal groups. In the lower molars the basic "Dryopithecus pattern" was visible, but it was overlaid, Gregory and Hellman noted, by a large and prominent "sixth cusp" and an "incipient 'plus pattern'" on the third lower molar in one of their fossils, and these were conditions characteristic of hominids.[241] The only difficulties regarding the molar teeth were their enormous size, a size which they said was "exceeded only by _Sivapithecus giganteus_ and by male gorillas," and the fact that the third upper molar was much larger in proportion to the first than in either "Sinanthropus" or _Homo_.[242] However, to the idea that this great size represented a specialization which would "definitely exclude the South African man-apes from direct ancestry to man," Gregory and

Hellman responded that "we have seen many indications that specializations may often be reduced and gradually replaced as if by the influence of newer and greater specializations."[243]

So much for individual teeth, but what of the overall form and proportions of the australopithecine upper and lower jaws? First, the authors' restoration of the upper dental arch of "Plesianthropus" produced a tooth row that was "posteriorly divergent and more man-like than ape-like in its breadth indices (relative widths across p , m , m , as compared to width across the canines)."[244] In their view this approach toward the so-called "parabolic" dental arch of humans provided a "structural link between those of the ancient Siwaliks stem ape, Sivapithecus sivalensis, and primitive man."[245] They also claimed that the reconstructed "premaxillo-maxillary region" of the female "Plesianthropus" face appeared to be "fundamentally similar to that of a certain female chimpanzee, except that its alveolar prognathism was less pronounced, the crowns of the lateral incisor and canine being directed downward rather than forward." This was a highly important difference since it fit in with the idea of the "edge to edge" bite that had already been alluded to with reference to the canine teeth. Similarly, Gregory and

Hellman portrayed the "moderate prognathism" of the more robust australopithecine "Paranthropus" as "structurally intermediate between existing apes and primitive man."[246]

Just as the shape of the dental arch seemed to approach hominid conditions, so also did the inferred pattern of mastication. Gregory and Hellman studied the wearing planes of the upper molars, premolars and canines in "Plesianthropus" and concluded that "the excursions of the mandible were on the whole more rotary and less obliquely transverse than in typical apes."[247] They also noted how the crowns of the lower molars in the South African fossils presented "nearly flat surfaces as in man, whereas those of apes show steep elevations, especially on the inner side." To them this fact implied that the "small size of the canines and the lowering of the cusps of the grinding teeth were making possible the hard-gripping, hard-grinding actions that are indicated in well-worn dentitions" of modern humans inured to a diet of "tough and gritty food."[248]

In emphasizing "transitional" and hominid features in the australopithecine dentition, Gregory and Hellman did not ignore important differences between the "man-apes" and other known hominids; none of these, however, shook their confidence in the close evolutionary relationship between humans and australopithecines. For example, in discussing the overall proportions of the upper dental arch they noted that the "relative length from the anterior face of the canine to the posterior face" of the third upper molar as compared to the width of the arch was of "more ape-like than human" dimensions. This feature, however, they attributed to a single factor they had already noted, the large size of the "man-apes'" grinding teeth.[249] They also invoked the same factor to explain the "forward protrusion of the upper and lower jaws" of "Plesianthropus." Though this prognathism was comparable in degree to that found in female chimpanzees it thus had a different cause -- it was "conditioned rather by the great anteroposterior extent of the molars rather than by an alveolar prognathism of the incisors."[250]

Another potential anomaly concerned the observation that "premaxillary prognathism" was actually much more pronounced in "Plesianthropus" than it was in a geologically older, but recently discovered Siwalik fossil, the so-called "Ramapithecus breviostris Lewis". If Ramapithecus were taken to represent the "progressive" line of dryopithecine evolution then the australopithecines might possibly have been too specialized in a pongid direction to have played a role in human emergence. While this interpretation was possible, the fact that Ramapithecus was a "much smaller and more delicately built form" than australopithecines, modern great apes or humans seemed to Gregory and Hellman to be a sufficient explanation for its lack of "prognathism;" the latter character was thus not the kind of evidence on which one ought to question the evolutionary role of the South African "man-apes."[251]

In making their own reconstruction of the skull of "Plesianthropus" Gregory and Hellman had to deal with other characteristics that were unlike then accepted hominids. Two that they emphasized were the "ape-like upward slope of the posterior portion of the bony palate" and the "marked downward pitch of the maxilla." But they were able to combine these with the other "ape-like" characters and the transitional and hominid-like characters noted above into a whole with "workable mechanisms."[252]

In one passage they went further, in order to speculate on how these mechanisms actually had functioned in living australopithecines in comparison with pongids and humans. "Modern apes," they wrote,[253]

> use their sharp canines to pierce and hold tough fruits, bamboo shoots, sugar cane, etc., which are cut into small bits by the more or less sharp crested molars. Primitive men use their small, almost incisor-like canines to grab and hold parts of the carcasses of animals, and their nearly

> flat-topped molars to grind flesh, small bones and grain. The South African man-apes were in an intermediate structural stage. As they lived in an open country which was much the same as it is today, they may have chased away the vultures and hyenas and filled themselves with the noisome remnants of the lions' feasts.

In connection with this notion of the "man-apes" as meat eaters they also took note of Dart's theory that the australopithecines might have broken open baboon skulls to get at the brains inside. Since even the digestive tract of the frugivorous gorilla was very similar to that of humans, Gregory and Hellman argued that it would not take much to transform a primitive hominoid from a fruit-eater into an omnivore that scavenged and/or hunted small game. "The transitional conditions in the dentition of the South African man-apes" thus could be seen as evidence of a "gradual shift from frugivorous to omnivorous food habits."[254]

Gregory and Hellman were thus able to make a strong case that the conditions they had observed in the australopithecine dentition harmonized perfectly with the "dietary hypothesis" about human origins that Greogry had maintained since 1916. The problem of the australopithecines' massive grinding teeth still remained, but that did not deter them from advancing some conclusions about the "systematic position" of the "man-apes." As to the relationships among the three genera already named by Broom and Dart, the American scientists were cautious "at least until more material" became available. "Paranthropus," however, seemed to differ obviously and in important ways from "Plesianthropus;" characters like the former's flattened "facial plate" and "excessive" jaw robustness seemed to them to justify Broom's placement of "Paranthropus" on a specialized side branch of the "pre-human stock."[255] On the relationship of the Taungs fossil to Broom's specimens Gregory and Hellman, while noting that this juvenile australopithecine presented "exactly the right general characters for the young stage of either _Plesianthropus_ or _Paranthropus,"_ judged that in some details of molar form it approached the Sterkfontein "Plesianthropus" fossils more closely.[256]

A more critical issue for the Americans than relationships among the australopithecines was the position of these creatures _vis à vis_ other hominoid groups. In assessing these relationships a great source of uncertainty was what they perceived to be the resemblance of the "man-apes" to modern orangs in several important characters -- for example, "the marked concavity of the lower facial profile," and "the general proportions and patterns of the upper and lower premolars and molars (except for the wrinkling of the crown surfaces in the orang)." The australopithecines were hardly unique in this regard, however, for Gregory and Hellman noted some important points of resemblance between orangs and certain hominids; for example, the molar teeth found near the "type skull" of "Pithecanthropus" had at least twice been identified as orang teeth, as had the lower jaw and teeth found with the cranium of "Eoanthropus." Modern orangs, they pointed out, had also achieved "a high degree of brachycephalism and hypsicephalism" in their skulls, as had "some of the Mongoloid peoples" though an identical mechanism -- i.e. through "arresting the growth of the basis cranii and accelerating the transverse and vertical growth components of the cranium and face." To Gregory and Hellman these "parallelisms" were not due to convergent evolution alone, as in "bulldogs and short-faced cats," but were "good evidence of remote genetic relationship."[257]

For all these reasons it seemed sensible not to leave the orang line out of the human evolutionary picture. Instead the multiple resemblances among australopithecines, fossil hominids, orangs and African great apes seemed to open up the possibility that[258]

> orang and man have diverged very profoundly and very rapidly from a more chimpanzee-like ancestor and that the Australopithecinae have such a mixture of characters because they were late survivors of the common *Dryopithecus* stock, and were truly related to all their cousins of the modern chimpanzee-gorilla,

orang and human branches.

This point of view was compatible with the body of data then available (data of widely varying value, of course), especially if one were committed as Gregory was to the importance of qualitative similarities in molar crown patterns in tracing dental evolution. However, it lent a kind of vagueness to the authors' conception of the actual timing of the dryopithecine radiation and the place of the australopithecines within it. The passage could be read in such a way as to include theories that orangs and humans last shared a common ancestor either before or after hominids split off from the chimpanzee-gorilla stock, as well as either before or after the australopithecines and primitive humans diverged from each other. In fact, it could be read to imply that australopithecines were no more nearly related to humans than they were to orangs. This would have rendered the "systematic position" of the "man-apes" little different in Gregory and Hellman's scheme than it was in those of writers who saw them as interesting anthropoids which managed to converge upon some important hominid characters.

That the latter evaluation was not intended, however, came through clearly when Gregory and Hellman discussed human-australopithecine relationships directly.

Indeed, they specifically ruled the idea out, claiming that it hardly did "justice to the numerous features in which it [in this case "Plesianthropus"] is transitional between the ape and human families." Given what they called an "astonishing mixture of ape and human characters" in "Plesianthropus" Gregory and Hellman confessed that they had been for a long time "in doubt whether to call it a very progressive ape or a very primitive man."[259] Actually, they decided to call it neither. What they did do was to state conclusively that "these South African Pleistocene man-apes were both in a structural and genetic sense the conservative cousins of the contemporary human branch." The word "conservative" referred to the late date then accepted for the age of the australopithecine fossils, for if the latter really were Pleistocene they seemed excessively primitive when compared to other Pleistocene hominids to have been true human ancestors.[260]

The term "cousins" was also a general one that seemed to leave the australopithecine position in the human family tree quite vague, but Gregory and Hellman narrowed down the range of options when they laid out a new version of Gregory's table of "structural stages" in the evolution of the human dentition. First, they included the _"Australopithecinae"_ as a full fledged stage between the "ancestral ape stock (Dryopithecinae)" and the "Homininae," and gave the "man-apes" the taxonomic status of a "new subfamily" among the primates. Second, they described the "Homininae" in a new way as "large-brained, omnivorous-carnivorous derivatives of the early australopithecine branch." Thus, Gregory was clearly arguing for the first time that human beings could expect to find some form of australopithecine in the chain of their direct ancestors.[261]

It is interesting that these conclusions, which to a modern eye seem to be the most important element of Gregory's and Hellman's research on the australopithecines, were only stated in a descriptive précis and not actively defended in the concluding remarks that followed. Instead the authors finished off with a passage on the broader lessons to be derived from the "man-apes." Specifically, they asserted that "all the facts known to date," including those which documented "the close structural approach of Plesianthropus toward Sinanthropus," tended to "confirm the conclusions of Davidson Black, Weinert and the present authors, who regard man as the result of a morphological revolution which took place during the later Tertiary period." They added, somewhat ruefully perhaps, that the facts on which this judgement rested (in their opinion firmly) probably would not shake the faith of those "who cling hopefully to the myth of Eocene man," even though that myth was based largely on "unproved assumptions of 'irreversibility', 'parallelism', etc."[262] In concluding on this by now habitual note, Gregory might be said to have neglected vigorous new trees for a largely overgrown forest; however, it is evident that the battles with Osborn et al. had put such a premium on the broad theoretical implications of the "man-apes" that the details of australopithecine phylogenetic relationships might easily be glossed over.

In his researches of the late 1930s Gregory developed a set of conclusions on australopithecine morphology and phylogenetic relationships that would change little in the years thereafter. He continued to take a strong interest in new data on the "man-apes," however, and he reflected further on the significance of these fossils in his late writings. Thus, in 1945 he and Hellman published a revised reconstruction of the "Plesianthropus" skull based on new information and casts provided by Broom. While their conclusions about dental morphology remained the same, details of the face and skull vault were pictured as slightly different from their earlier conceptions of them. More revealing, though, were the differences that Gregory and Hellman found between their revised reconstruction and Broom's. When Gregory and Hellman compared their measurements of the characters involved to his, they found "in most respects" a consistent pattern -- their numbers for "Plesianthropus" lay "between anthropoid and human limits," while those of Broom tended to "lie within the anthropoid" range.[263]

Gregory's insistence that the australopithecines were a transitional hominid form that in many ways bridged the gap between dryopithecines and more human-like Pleistocene fossils also informed his last major article directed specifically to the problem of human evolution -- a 1949 essay entitled "The Bearing of the Australopithecinae Upon the Problem of Man's Place in Nature."[264] In this essay the main themes of his earlier work on human emergence were repeated -- especially the crucial significance of locomotor adaptations in "transforming" progressive anthropoids into primitive hominids, the hominid affinities of the australopithecine dentition, and the notion that the latter contained evidence of the shift from frugivorous to "omnivorous-carnivorous" diet. The only significant new pieces of evidence that Gregory added related to the question of posture and locomotion; on this issue he asserted that a) there was now more proof than before that the australopithecine skull had been balanced atop the vertebral column in a human-like fashion, and b) the talus (the main ankle bone) of the "man-apes" was much nearer in form to that of Homo sapiens than to quadrupedal forms such as the gorilla. Both these findings strengthened his belief that the South African fossils represented a transitional stage in the shift from brachiation to terrestrial bipedalism.[265]

Providing new evidence or new conclusions was not Gregory's real aim in the essay; rather, he saw it as a chance to sum up and meditate on the meaning of hypotheses he had long found compelling. In the process he gave a retrospective on some of the historical factors that had influenced the debate about human origins in his years as a scientist. Most interesting and important in his opinion were the sorts of "basic assumptions" about human history and the evolutionary process that biologists commonly adopted, assumptions through which new data seemed to be filtered and thus all too easily ignored. It seemed to him that one could correctly interpret the significance of the australopithecines in particular only if one examined the questions implicit in these assumptions and resolved them.[266]

As was his habit in discussing matters of theory, Gregory presented these "basic assumptions" in pairs; none were strictly "either-or" propositions, but rather extremes or poles on a continuum. Though not the overt subject of discussion, Gregory's own position within each continuum became obvious quickly. The first opposition he called that between "teleologists" and "epigenesists." The former, he argued, tended to portray humans as constructed in accordance with a "plan" present from the origin of the hominid line. This notion was obviously present in creationists, especially fundamentalist Christians, but some evolutionists seemed to be "teleologists" also, for example, those who were rigid "irreversibilitarians" in their conception of the evolutionary process. "Epigenesists" by contrast thought of evolution as a more open process, one less bound to adhere to a limited set of "plans." For them, evolution could proceed through directional change in "old" features and thus develop a basic plan further; it could just as easily proceed, however, through the elimination or reduction of "old" features and the emergence of new patterns at the "habitus" level, and thereby bring new "plans" into being.[267]

Like the first, the second contrast had also been a familiar one in Gregory's previous writings -- i.e. the division between "pithecophilians" and "pithecophobiacs," or those who seemed to be comfortable with anthropoid human ancestors and those who did not. In this essay, though, he pointed out more clearly than in previous discussions that these categories were not simple and homogeneous. Differing shades of opinion existed within each group. For example, he noted, Robert Broom could stress both the "ape-like" characters of the australopithecines and their close genetic relationship with humans, and yet reject the idea that either australopithecines or humans had been derived from the "proto-anthropoid" dryopithecines.[268] Another major division among "pithecophiles" concerned whether humans were descended from a large-bodied "brachiating" ancestor, and thus over humans' degree of relationship with what he termed the "chimpanzee-gorilla stock." As examples of anti-brachiators he singled out W.L. Straus, who (as we have seen) emphasized the "primitive" characters that humans shared with pronograde monkeys and not with pongids, and the British anatomist W.E. LeGros Clark (1895-1971), who conceived of the dryopithecine Proconsul africanus as closely allied to the as yet undiscovered first hominids but argued that neither was likely to have been a "brachiator."[269]

Gregory did not attempt a general analysis of the reasons why some writers liked to look for the apes in the human family tree while others seemed bent on shaking them out; he also ignored the influence of things like personal temperament, religious or ethical principles, and socially-based conceptions of human nature. He did, however, focus attention on more narrowly scientific preconceptions that seemed to underlie the choice of the other "basic assumptions" noted above, and thus helped determine whether people would be "pithecophiles" or "pithecophobes." Thus, those whom he called "vectorians" -- those who believed that long, distinct and often parallel lines were the normal mode of evolution -- were generally "pithecophobes," while "cladogenists" -- those who, like Darwin, saw branching patterns of adaptive radiation as all-important -- were normally "pithecophiles." Though he did not point them out, other associations were also implicit in these categories, i.e. between "vectorians" and "teleologists," as well as between "cladogenists" and "epigenesists." It was also obvious where Gregory's own allegiances lay, since most of his work on "basic patents" and "transformations" was dedicated to counteracting "vectorian" tendencies.[270]

As his final "dichotomy" Gregory revived another issue that had occupied him often since 1916 -- the conflict between "homunculists" and those who rejected this concept of early hominid ancestors. In addition to believing in very "manlike" ancestors divergent from the main primate stem at a very early date, homunculists, he said, tended to be "vectorians" and "irreversibilitarians" too. All these attitudes worked together so efficiently that such thinkers could almost be counted upon to deny ancestral status to any possible hominid that retained the least hints of anthropoid "specialization." So strong were their preconceptions, he felt, that apparently no amount of negative evidence could shake their contention that "true" human ancestors would one day be found.[271]

For Gregory, then, the manifold resemblances between the australopithecines and generally accepted hominid forms as well as their transitional status between the hominid and pongid groups could only receive a satisfactory explanation if one were prepared to drop "homunculism," "teleology" and "pithecophobia" and embrace the views of the "cladogenists," "epigenesists" and "pithecophiles." On this view, nature was not a rigid follower of "plans" but a "pragmatist;" changes in evolutionary direction could and did occur often, though they were constrained "within the limits imposed by the varying incidence of hereditary factors and by changing selective pressures."[272] Interpreted according to these principles the australopithecines could not fail to remind humans of their anthropoid ancestry, and more broadly of their common heritage with the "lower" animals.[273]

From the foregoing it is clear that while he paid careful attention to matters of detail concerning the australopithecines, Gregory's main interest was in using the group as a whole for the support that it could give to his long held views on human emergence. Such an emphasis was not unusual in 1949, for he was at a stage in his scientific career when it was natural to reflect upon the debates that had given shape to his work on human evolution and to use the new evidence to vindicate his previous conclusions. Indeed, the article on the australopithecines was an anticipation of a vastly more extensive effort along the same lines, the book _Evolution Emerging._[274]

Evolution Emerging: Capstone of a Career

By Gregory's own account, his _magnum opus_ -- which ran to 560 pages of double-columned text with 1000 pages of illustrations in a separate volume -- was the final result of a twenty year project. More even than this however, it summed up his entire life's work in the study of comparative anatomy and paleontology. In his preface Gregory noted that the idea of a comprehensive volume on vertebrate evolution had actually germinated among the members of the Columbia University zoology department another two decades prior to 1931, the year in which Gregory himself had taken up the task. Osborn was to have been the author, but he had been sidetracked. After Osborn had passed the project on the his protégé, it matured slowly; as the years progressed, Gregory compiled notes upon notes, feeling the need, as he explained it, not only to follow as many of the branching paths of vertebrate evolution as possible, but also to assess the basic evolutionary and ecological relationships between vertebrates and invertebrates.[275]

In the completed treatise Gregory tried, in his own words, to "avoid descriptive detail as an end in itself" in favor of talking about "individuals as representatives of genera." Thus, while the anatomical discussion was often detailed, it was meant to illustrate broad evolutionary relationships, with particular emphasis on "the emergence of new skeletal patterns;" the latter he examined especially for the evidence they could give about "adaptations in body form, [the] locomotor system, and in the organs for seizing, subdividing and ingesting food."[276]

The methods that Gregory employed were, as always, the traditional ones of descriptive morphology. He made no apologies for his continued allegiance to these methods, and indeed he defended them against those people who would allege that science's primary concern was with things that could be quantified. Thus, he asserted,[277]

> science in which the interpretive function is the end and measurement the means is often thought of as limited to quantitative determinations. It was not by measurements, however, that the riddle of the Rosetta stone was deciphered, but by the method of matching like with like, of starting with the known and gradually decoding the unknown. This was the method used by Darwin in his great delineation of the outlines of evolution and it has continued to yield abundant results.

It is difficult to do justice to the panorama represented in Evolution Emerging. After his introductory survey of the invertebrates, he went on to give decriptions of all the major families among fishes, amphibians, reptiles, birds, and mammals, both fossil and recent. As in his earlier writings, however, he singled out for the closest analysis creatures that displayed distinctive and/or highly successful adaptive patterns, and especially those that exemplified the "basic patents" critical to the eventual emergence of humanity. Even to follow out the lines of Gregory's discussion of the latter issues alone would take us beyond the scope of the present essay; the principal concern here must be, rather, with Gregory's accounts of higher primate evolution, of human origins, and of the theoretical views that informed the whole work. Since Evolution Emerging truly encapsulated a life's work, a full discussion of how the book handled these issues would entail repetition of themes developed earlier. Still, there were new pieces of evidence and changes of emphasis in Gregory's account of human phylogeny that were noteworthy, as was the precise fashion in which he restated some of his principal theoretical concerns.

As might be expected from the trend of the preceding two decades, the australopithecines received special attention in Evolution Emerging. New evidence had appeared on what was to him the main focus of attention, the dentition, in particular from the fossils discovered at the South African site of Makapansgat and called "Australopithecus prometheus" by Dart. In addition there was more evidence of erect posture in the "man apes," especially a "small pelvis of human or subhuman type, with transversely widened ilia." Finally, there were the suggestive (if highly speculative) descriptions of australopithecine "braincasts" published by Broom's colleague, the neuroanatomist G.W.H. Schepers (b. 1914); while he did not make use of all of Schepers' arguments Gregory did accept the contentions that the australopithecine brain was more hominid-like than ape-like in the form of its frontal lobes and that the latter fact implied "articulate speech was very probably near its beginning in Australopithecus." [278]

The phylogenetic conclusions Gregory drew from the accumulated evidence on the australopithecines were also put in a stronger form than he had been willing to do a decade earlier. Now there was no equivocation at all over the fact that humans were more closely related to the "man-apes" than to any other primates. Though the problem of geological age was still important, forcing him to grant that "the known Australopithecinae may be the great-uncles rather than the great-grandfathers of man," Gregory was certain of his morphological judgement -- i.e. that the australopithecines were "structurally intermediate between the older anthropoid stocks and the subhuman types represented by Meganthropus, Pithecanthropus and Sinanthropus." [270] As before, he was also willing to attribute these structral similarities to common descent, arguing that both the Australopithecinae and humans were "of probable lower Pliocene or upper Miocene derivation from some one of several genera of the Dryopithecinae."[280]

Another important feature of Evolution Emerging was Gregory's discussion of those Pleistocene hominids which were much closer to modern humans than the "man apes" of South Africa, since he had not treated this subject in detail since the 1920s. The great accumulation of new fossils since that time was apparently not sufficientlly clear cut in implication for his taste, for he shied away from advancing any particular scheme of relationships among the various fossil human forms. Instead he contented himself with pointing out salient characters of individual fossils that seemed to have significance for his scenario of human emergence as a whole.

If by nothing else than by the number of times he was cited, it was clear that Franz Weidenreich (1873-1948) had set the terms for much of Gregory's discussion of Pleistocene hominids. Gregory had immense respect for the German scientist's work in paleoanthropology, and in fact had played a role in securing for the latter an appointment at the American Museum when the Japanese invasion had made the continuation of his work in Peking impossible.[281] Not surprisingly, Weidenreich's influence was most evident in Gregory's remarks on the two Asian varieties of lower to middle Pleistocene _Homo erectus,_ which he continued to call by their old names "Pithecanthropus" and "Sinanthropus." Two conclusions about these fossils made by Weidenreich were particularly meaningful to him: first, that the two forms were closely related and represented roughly the same stage in human evolution, and second, that some of the unqiue characteristics of the "Sinanthropus" dentition were retained in that of the modern population of north China and suggested genetic derivation of the latter from the former "at least in part."[282]

While Weidenreich's opinion carried weight with Gregory, it should not be seen as the only factor in Gregory's re-evaluation of Homo erectus. Indeed, it could be argued that Gregory's receptiveness to that opinion was largely determined by its congeniality with his general view of the ape-into-human "transformation." In their cranial architecture and their degree of dental reduction both forms of Homo erectus made perfect intermediates between the australopithecines and modern humans. By accepting Weidenreich's characterization of them he was moving back toward a position that he had first entertained regarding the erectus- like Mauer jaw in 1916 -- i.e. that morphologically robust early Pleistocene hominids had played a direct and important role in human evolution.[283]

Another issue raised by Weidenreich in which Gregory showed a great deal of interest was the former's theory of "giant early man," but in this case he was not so quick to make an endorsement. Gregory discussed this theory in connection with a curious fossil that Weidenreich had made a great deal of -- the so-called "Meganthropus paleojavanicus" discovered in 1941 and described first by the paleoanthropologist Ralph von Koenigswald (b.1902).[284] The massive size of the "Meganthropus" jaw, which nevertheless had teeth of hominid affinities, together with the huge dimensions of the molar teeth of the Chinese fossil Gigantopithecus (which Weidenreich also considered to be hominid in form), had prompted the latter to theorize that early Pleistocene human ancestors had been a race of giants; this great body size, Weidenreich contended, had provided the essential impetus for pushing the hominid brain beyond anthropoid dimensions.[285]

While the "giant early man" theory was attractive to him, perhaps because it squared with his original view of early hominids as ferocious hunting creatures,[286] Gregory felt compelled to withhold judgement. In Weidenreich's favor he noted that the massive australopithecine lower jaws found at Swartkrans and named "Paranthropus crassidens" by Broom lent credibility to the theory, if one accepted (as Weidenreich apparently did not) the notion that there was a close evolutionary relationship between the South African "man-apes" and "the East Asiatic, Malayan Pithecanthropus - Meganthropus group."[287] However, he would not consider the "giant early man" idea as solidly based since "definite associations between very large skulls or jaws and large limb bones" were lacking.

In fact, while massive jaw bones had been found in South Africa, the only australopithecine limb bones then extant indicated creatures with "the proportions of pygmies." Gregory acknowledged the possibility that these bones might belong only to "small females" in a highly dimorphic species; it could also have been that both "physical extremes" had been present among males and females. A situation similar to the latter, he asserted, had apparently existed among the much less ancient population of Mt. Carmel; that group had included both large and small individuals, those of small stature having great similarities to the European Neanderthals, and the taller individuals not possessing them.[288] The verdict, then, on giantism in the lower Pleistocene was a "definite maybe" -- Gregory concluded that Weidenreich's was a "challenging thesis," and predicted (incorrectly) that it would provide a "major incentive for further exploration and discovery for anthropologists for several decades to come."[289]

Gregory also tried to make some (though perhaps not enough) use of Weidenreich's ideas to resolve the most difficult problem that confronted him in his analysis of Pleistocene hominid evolution -- the question of the first appearance of _Homo sapiens_ itself. As it had during the

1920s, Gregory's acceptance of the authenticity of the Piltdown skull was a major stumbling block. Whatever the final disposition of the jaw of "Eoanthropus" (Weidenreich had of course made a very strong case that it had belonged to an orang[290]), Gregory still believed that the skull indicated the existence of "a thick-skulled, large-brained man present in England, the contemporary of many now extinct mammals of Pleistocene age."[291] That this skull represented _Homo sapiens_ itself he did not want to accept, however, and so he minimized the differences between Piltdown and other primitive hominids. Thus, in a passage on the juvenile skull of _Homo erectus_ that von Koenigswald had named "Homo modjokertensis," Gregory went on to speculate that[292]

> the subglobose form of cranium which is found in infant anthropoids was already becoming evident in several human skulls in early or mid-Pleistocene times, and especially in modern man it is to some extent a retained infantile feature. Hence it is possible that too high a systematic value has been put on the presence or absence of the retreating forehead and supraorbital torus in man.

As a way of both accepting the Piltdown skull and a "smooth-browed" reconstruction of Swanscombe Man (the "mid-Pleistocene" skull referred to in the passage) without endorsing an "early _sapiens_ " theory that would exclude _Homo erectus_ as a human ancestor, Gregory's suggestion met his immediate needs. However, this solution to the "brow ridge problem" when viewed in its context seemed just as weak, or even weaker than his earlier attempts. For by 1951 the preponderance of hominid fossils dated earlier than the late Pleistocene which possessed a well-developed supraorbital torus was far greater than it had been during the 1920s. In addition, the assumption of individual variability and lack of "systematic value" for such a well-marked character seemed to conflict with the presumption that important structures possessed adaptational significance as well, a presumption which was fundamental to the tone and style of argument of Gregory's entire body of work.

Gregory would have been far less likely to venture so uncharacteristic a hypothesis had it not been for the evidence of the Swanscombe skull, as interpreted by the British scientists who had first subjected it to close study. As it had for Hooton, the discovery of a mid-Pleistocene skull of "the smooth, well-rounded type, with a relatively large, well-convoluted brain" seemed to confirm the importance of the Piltdown skull and to render the early _sapiens_ theory a great deal more secure.[293] In fact, Gregory tried to add some support for a modified version of the early _sapiens_ theory by putting the Swanscombe findings into a broader mammalian context. Thus he claimed that having _Homo sapiens_ date back to the

"Mindel-Riss" Interglacial epoch (the date agreed upon for the Swanscombe deposits) would come as "no surprise to vertebrate paleontologists, who are familiar with the evidence that the latest, or almost latest, stages of evolution of many mammals had been reached in the late Pliocene or Pleistocene epochs."[294]

The obvious difficulty with this bit of "Osbornian" reasoning was that it left Gregory with the onerous task of accounting for the most numerous group of mid- to later Pleistocene hominids, the "Neanderthaloids," a problem that had plagued his account of hominid evolution from 1920 on. As earlier, he failed to confront the questions of why the "Neanderthaloids" had been left behind in an evolutionary sense and how they managed to coexist so long with morphologically more "advanced" forms of humanity. Instead, he attempted to preserve the evolutionary importance of both "early sapiens "and the "Neanderhtaloids" by invoking the tried and true notion of a Neanderthaloid "structural stage" in human evolution, this time using Weidenreich's researches as his base. Thus, in Gregory's view, Weidenreich had demonstrated "fully" that, while early Homo sapiens and the "Neanderthal race" were contemporaries, this fact could not "neutralize the evidence of comparative osteology that the Neanderthal stage contained many relatively primitive features which were gradually lost in the more advanced members of <u>Homo sapiens</u>."[295]

Gregory ended his discussion of fossil hominids, then, on a note that would have seemed half-hearted to those who were after a clear account of the relationships among the various Pleistocene "races" that he had described. This lack of precision, however, was fundamentally in keeping with the priorities expressed in <u>Evolution Emerging</u>, and with Gregory's study of primate evolution as a whole. As always, the paramount issue was not the later stages of human emergence but the relationship between humans and apes -- with the dryopithecines on one side, and the "chimpanzee-gorilla stock" on the other.

Because Gregory's aim in discussing the dryopithecines was that of elucidating their relationship with hominids, he did not emphasize differences among the various dryopithecine genera and species in <u>Evolution Emerging</u>. He also singled out for special attention the dryopithecine fossil that best suited his purposes -- <u>Proconsul africanus</u>. What made <u>Proconsul</u> particularly interesting was its early date, Lower Miocene, and its combination of "primitive" skeletal characters retained from such earlier anthropoids as <u>Propliopithecus</u> and new features that seemed to Gregory to be ancestral to characters found in modern chimpanzees. The sort of "generalized" dryopithecine represented by Proconsul, he suggested, provided a fair starting point from which the hominid line could easily have evolved. "Most authors," he noted,

> would probably assume that Proconsul could not be ancestral to man because it displays so few unequivocally human characteristics; but that may well be because, at the relatively remote period (lower Miocene) in which Proconsul lived, distinctively human characters had not begun to be differentiated from primitive ape characters.[296]

In regard to employment as Miocene human ancestors, Gregory's motto was still "homunculi need not apply".

The willingness to suggest this sort of role for Proconsul was of course related to another persistent theme -- the very close evolutionary relationship between humans and the living African great apes. Just as Proconsul, the putative ancestor of the chimpanzee, stood in for the "generalized" dryopithecine type, so also did the chimpanzee seem to Gregory to be the modern form retaining the closest resemblance to the main dryopithecine stock. Unlike the later Hooton, he apparently felt little need to compromise with the "anti-brachiators." "Fortunately," he asserted, "the chimpanzee preserves for us what appears to be a fairly central anthropoid type of body, which avoids the excessively long forearms and very long hands of the orang and the gigantism of the gorilla."[297] By promoting the close relationship between chimpanzees and humans, and the former's special status as the "least modified of the modern descendants of the Dryopithecinae," Gregory was not, however, contending that the chimpanzee had an exclusive claim as humanity's closest living relative. He still continued to stress the fundamental structural similarities between gorillas and humans as well, especially in the brain and other internal organs.[298]

In this, his final defense of the "ape-man" theory, Gregory could also not resist beating what must have been by then a truly dead horse, Frederic Wood Jones' notion that the cranial architecture of humans evinced a "tarsioid" and not an "anthropoid" ancestry. This time, he described the differences in sutural patterns between humans and their anthropoid cousins as results in the latter group of "stiffening systems" which had evolved in the skull bones along with the massive nuchal and masticatory muscles required by large-bodied adult apes. These differences were thus in "habitus" characters, and in the years since 1934 the state of the evidence had not changed.[299]

Gregory's final summation of the comparative anatomical evidence relating to human evolution revealed, just as the example above, that he had changed none of the principles he had defended between 1927 and 1934. Reduced to its essentials, his message still was as follows: human beings had evolved from a "primitive" or "generalized" brachiating anthropoid ape of the dryopithecine group by means of "transformations" in a whole complex of characters connected with the locomotor, dietary and behavioral adaptations that distinguished the human "habitus" from that of its African cousins. The list of these changes -- altered proportions in the leg bones, shoulder girdle and pelvis, rearrangement of bones and muscles in the foot and hand, etc. -- would also have been completely familiar to those acquainted with his earlier writings.[300] He obviously saw no need to change, for, as noted above, with the evidence provided by the australopithecines, "Sinanthropus," etc. the fundamental truth of "transformation" seemed more securely grounded than ever.

Evolution Emerging was not just Gregory's attempt to pull together his specific phylogenetic judgements on the evolution of primates and other vertebrate groups; he also wanted to explore the general principles at work in the evolutionary process one last time. The focus in these theoretical passages was on the pairs of complementary principles discussed earlier in the chapter -- "polyisomerism" and "anisomerism," "transformation" and "undeviating trend," and of course "habitus" and "heritage."[301] Gregory even waxed poetic about these grand evolutionary themes in his introductory chapter, and produced a three-page "argument" in blank verse with stanzas like the following:[302]

XII
Of earlier forms, the habitus, or mask,
That fits them for a special way of live,
To all their seed becomes prerequisite,
The basic portion of their heritage --
"Preadaptation" but not predestination.

XIII
And yet the later habitus conceals
Part of the "total heritage," as when bats
Flying like birds, are proven to the mammals.
Thus heritage and habitus intertwine.

While the tone of the verses was that of the 18th century, the content was still self-consciously Darwinian. In Evolution Emerging Gregory continued to portray "Natural Selection, operating upon the products of secular genetic changes" as the main motive force in the evolutionary process.[303] In fact, passages near the end of the book indicated both an awareness of and an attempt to incorporate into his argument ideas from the neo-Darwinian "new synthesis" that was coming to dominate the biological sciences in the post-World War II era. For example, in summarizing the genetic processes that underlay evolutionary change in morphology, Gregory asserted that variation arose both from random mutation and from the "shuffling" of genes, and cited both natural selection and isolation as the means by which these genetic variations spread through "breeding populations." He also made use of modern genetic ideas to defend the theory that "primitive" anthropoids had been gradually "transformed" into hominids. To those who alleged that such an extensive series of correlated morphological changes was unlikely, he replied that "as Sewall Wright and others" had shown,[304]

> there is a well-founded statistical basis for the assumption that when, in the history of any given evolutionary series environmental opportunities and penalties happen to favor certain combinations of desirable improvements (as in brain, teeth, and limbs), such combinations do occur and continue over long periods of time.

Though they were neither extensive nor rigorous, these references to the emerging "new dispensation" in evolutionary biology provide a fitting place from which to conclude our analysis of Gregory's work in paleoanthropology, for they reveal to us its remarkable strength and coherence with great effectiveness. For though he often spoke in terms that were fast becoming outmoded -- with his liberal granting of specific and generic rank to fossil forms, distrust of statistical methods of morphological analysis, and heavy reliance on representative individuals to define "types" -- his fundamental position on critical interpretive issues like the relationship of humans to the other primates and the means by which hominid evolution had come about had clearly retained their vitality across a span of thirty-five years.

Gregory would have been the first to admit that, with the great and critically important exception of his studies on the primate dentition, most of his work on the problem of human evolution represented creative synthesis rather than original research. Indeed, more than once he decribed his own role as attempting to keep the vital insights of Darwin and Huxley fresh and in tune with the accumulation of new evidence. As the preceding analysis of Gregory's involvement in controversies about human evolution makes clear, the task of defending and advancing the Darwinian perspective in the years between the world wars was an absorbing one. Since he was the only American scientist of the era to shoulder this burden consciously and fully, he must in large part be considered responsible for the ease with which the post-war generation could erect its own structure along "neo-Darwinian" lines. The task of "slum clearance" had been accomplished, and an acceptable foundation dug.

A final, and valuable lesson taught by Gregory's oeuvre is the insight it gives into the great resilience of traditional methods of decriptive morphology in evolutionary biology. By the early 1950s much was being written by American physical anthropologists about the "old" descriptive versus the "new" experimental methods in that science.[305] Though perhaps not fully intentional, the invidious distinction implied by this terminology tends to mask the fact that there is a strong element of the "historical" along with the "experimental" type of science in the study of evolution. Experimetal methods can and do produce fundamental, and probably the more trustworthy, kind of insights, but they cannot render obsolete the "comparison of like with like" and the reasoning by analogies of past with present conditons which were the hallmarks of Gregory's scientific style.[306] If one remained, as he did, wary of unnecessary theoretical assumptions (and especialy of "laws" of evolution), and took care not to overinterpret isolated parts of the total evidence available, these methods could continue to produce results throughout a long career.

The combination of "old-fashioned" methods and self-conscious identification with traditional wisdom in Gregory can easily lead one to underestimate the value of his contribution to the study of human evolution. Yet in making that evaluation one must keep in mind these words of T.S. Eliot, which perhaps apply even more to the scientific than they do to the literary world: "Someone said: 'The dead writers are remote from us because we _know_ so much more than they did.' Precisely, and they are that which we know."[307]

CONCLUSION

As the foregoing chapters amply demonstrate, the period between the two world wars was a difficult one for the study of paleoanthropology in the United States. The discipline, as well as those that impinged upon it like vertebrate paleontology, prehistoric archeology and skeletal biology, was affected strongly by concepts that had reached dead ends of various sorts. In some cases either a semi-underground sympathy for Lamarckian processes or a more open adherence to a belief in orthogenetic, progressive change reflected an inability to explain human evolution in terms that derived from testable biological theories. Even a writer who was suspicious of both these patterns of thought, such as Hooton, could find little to replace them, and retreated to discussions of hominid phylogeny, especially its later stages, that eschewed analysis of the crucial issue of adaptation. The common habit of discussing human behavioral evolution in static, psychological terms also betokened a conceptual impasse in dealing with processes that involved interaction between the morphology and behavior of past hominid populations. In particular, the Darwinian processes of chance variation and natural selection were almost universally deemed insufficient to account for the "progressive" aspects of hominid evolution.

More prevalent in the discipline were the various habits associated with "typological thinking." Even among physical anthropologists like Hooton and Hrdlicka who were aware of the problem of variation, the idealized "types" generated by traditional wisdom, previous research or the practiced eye of the scientist himself often became reified, especially when they dealt with "primitive types" like "Pithecanthropus." The well-known "splitting" tendency that was thus encouraged in hominid taxonomy created confusion in the tracing of hominid phylogenetic relationships. And once "Humpty-Dumpty" was apart, reified "types" were all too often set free from their temporal context to be "put back together" in arbitrary arrangements of "structural ancestors." In the style of prehistoric archeology practiced by MacCurdy, the definition of cultural epochs on the basis of typological criteria became an end in itself, one so absorbing that it inhibited the systematic study of Paleolithic artifacts as indices of behavioral evolution.

In addition, we have seen that typological thinking made a resort to analogies with modern racial "types" almost unavoidable. In the case of writers like MacCurdy and Osborn the paradigm of race conflict became a reflexive way of dealing with the Neanderthals and other supposed "dead-ends" in hominid evolution. For Hooton analogies with morphological relationships among modern "races" reinforced a tendency to accept "multilinearity" and "asymmetry" as givens in hominid evolution. Finally, the habit of attributing particular "cultures" to particular "races" often absolved writers of the need to treat Paleolithic technologies as forms of adaptation that needed to be analyzed.

In tandem with these more general patterns of thought the period witnessed the ascendancy of several specific theories of hominid evolution that came into increasing conflict with the weight of the fossil and cultural evidence as time went on. The theories that the brain had played the leading role in hominid emergence from the primate stock, and that the course of later hominid evolution could best be explained by recourse to two, or more, parallel phyla, were both reasonable ones that were firmly rooted in careful, though perhaps flawed, empirical research. Nevertheless, their longevity was determined not so much by their ingenuity as by the mutual reinforcement between them and the general ideas noted above. For most of their adherents these ideas also made it possible both to profess a strong commitment to evolutionism, and yet to maintain the spiritual distance between Homo sapiens and its antecedents that they seemed implicitly to require. In some cases these theories had the further benefit of confirming cherished social attitudes; the racial implications of Osborn's "Pro-Dawn man" theory are an obvious case in point, but Hooton's "initiative" theory and "multilinearity" illustrate this as well.

Though the majority of paleoanthropologists espoused moderate versions of both the "brain first" and "parallel phyla" theories, extreme versions, such as the "Pro-Dawn man" theory, received considerable attention. While extremist views never achieved wide acceptance, it would be a mistake to dismiss them as unimportant; a great deal of energy was expended by writers like Gregory and Hooton in criticizing them, in large part because the latter realized their conformity with widely held assumptions. Though extreme, these "pithecophobe" scenarios also were symptomatic of a widespread dissatisfaction with the Darwinian orthodoxy, reinvigorated early in the period by Keith and Gregory, that humans had shared a recent common ancestor with the African great apes.

Another short-lived, but nevertheless important, phenomenon of the period was the attempt to provide convincing theories of hominid dispersal based on one or another "center" of evolution. Osborn's central Asian theory, it is true, relied overmuch on orthogenesis, speculative analogies with other mammalian phyla, and a mistaken theory of past climatic patterns and continental positions; Hrdlicka's Europe-centered theory put too much emphasis on a temporary lack of fossil and artifactual evidence from less-explored regions in Africa and Asia. Both, however, are highly interesting; if for no other reason they remind us that evolutionary scenarios often reflect the deeply held social and philosophical views of their propounders, and thus can be rendered highly resistant to contrary evidence. Additionally, however, they contained important hypotheses about the ecological settings in which hominids supposedly evolved at a time in the history of paleoanthropology when such questions were often side-stepped.

Partly cause and partly effect of the longevity of these theoretical trends was the confusion engendered by fraudulent or mistaken pieces of "evidence." The most important instance of the former category was of course Piltdown man, and of the latter Boule's reconstruction of Neanderthal posture. Once established in the literature these mistakes bred or confirmed expectations that influenced the interpretation of other finds like those of Swanscombe, Choukoutien and Mount Carmel profoundly. In the writings of the majority the total pattern of Pleistocene hominid phylogeny that resulted indeed resembled the "blind alley" discerned and decried by Hrdlicka.

The great contrast between Hrdlicka and Osborn on the Neanderthal question alluded to above also reminds us of another highly significant aspect of American paleoanthropology between World Wars I and II -- the great variety that coexisted with the more or less common interpretive themes noted above. Nearly every rule of the period had its exception, and all the major issues were open to debate. For this reason overarching "paradigms" are difficult to identify in the discipline as a whole -- there was no "American school" of interpretation of hominid or higher primate phylogeny. On the key problem of Neanderthal man, for example, opinion ranged from the extreme replacement theory of Osborn to the basically monophyletic "Neanderthal phase" theory of Hrdlicka. Gregory showed a continuing, if hesitant, attraction toward a "Neanderthaloid" structural stage in human evolution, and Hooton, despite his "die-hard Toryism" concerning "multilinearity" never rejected the notion that the Neanderthals had made genetic contributions to anatomically modern _Homo sapiens._ While the leading "generalists" like Hooton and Gregory remained wedded to the idea of a "brachiating" ancestry for humans, comparative anatomists such as Schultz and Straus subjected the theory to criticism.

The fluidity of opinion in the United States very likely had a strong influence on the relative speed with which the ideas of the neo-Darwinian "new synthesis" were absorbed by paleoanthropologists after World War II. Generational change and the increasing number of anomalies between the major theories of the interwar years and the fossil evidence also promoted the search for new approaches. Still, the fact that so many important questions had never been seen as settled meant that there was less to be "unlearned" before the new dispensation could take hold.

As is often the case in the history of ideas, there were also deeper continuities between old and new than would at first appear. In the area of methods one sees the beginnings of a concern with ranges of variation and their implications in the work of Hooton and Hrdlicka; though neither was able to dispense with the "typological" approach in his work on fossil and present day human populations, both pointed to the need for larger samples and quantitative analysis of the same. In Hrdlicka's work on the Neanderthal question and on hominid dental evolution these concerns paid valuable dividends.[1] Just as important was the concern shown by Hooton for incorporating a wide range of studies of non-human primates into his account of human evolution. In particular, his call for studies of primate social behavior in the wild and attempts to generalize from those that had been done clearly anticipated latter developments in a field in which Americans have played an important role.

In the realm of interpretation there were important continuities as well. Hrdlicka's concern with biocultural adaptation in the transition between Neanderthals and anatomically modern human populations (and in the continuing evolution of the latter) provides a critical example. Whether there was a direct influence upon later ideas is difficult to say; a strong case can be made that the work of Franz Weidenreich and the theoretical use made of it by biological proponents of neo-Darwinism like Ernst Mayr and Theodosius Dobzhansky really launched this trend in the postwar era.[2] Still, it seems unquestionable that the ideas of Hrdlicka, and Gregory, who arrived independently at an account of hominid evolution that stressed the role of dietary factors on the biocultural evolution of humankind, made the application of the views of Mayr et al. a great deal easier for American paleoanthropologists.

The name of Gregory is also connected with two other interpretive positions of continuing importance in

America. The more general, and one which he shared in part with Hooton, is what he liked to call "pithecophilia" -- that is, emphasis on the qualitative similarities between humans and the existing great apes and their close phylogenetic relationship. It might be argued that the degree to which he felt the need to combat "pithecophobia" caused Gregory to be dogmatic in his adherence to the "brachiating ancestor" hypothesis; whether he was correct in his version of the theory or not, his vigorous defense of "ape-men" in the human family tree was a major legacy that later American paleoanthropologists like Sherwood L. Washburn have been conscious of.

Also, as wé have seen, it was not coincidental that the champion of the "ape-men" also became one of the first students of primate evolution of international stature to insist upon the hominid status of the "man-apes" of South Africa. The success of "pithecophilia" and scientific support for the australopithecines went hand in hand, as Gregory was aware. That study of the latter group became such a dominant concern among American paleoanthropologists owed a great deal to Gregory's prior efforts in behalf of both "man-apes" and "ape-men."

The period between World War I and the 1940s thus presents a complex pattern of continuities and discontinuities with the era that followed. The impact of "population thinking," rigorous quantitative methods and ecological approaches to the study of evolution that accompanied the "new synthesis" in the 1950s and 1960s clearly altered the paleoanthopological landscape dramatically.[3] Nevertheless there were a great many concepts in the work of the older generation that could be, and indeed were used as points of departure by the younger. The record thus provides an illustration of the point made by Michael Ruse in discussing more recent debates, that "paradigm shifts" in evolutionary biology are rarely as abrupt as the common understanding of that term would imply.[4]

If even some of these observations about major figures in American paleoanthropology can be applied to the history of the discipline as a whole, it appears that further analysis of this history will have value both for historians of science and social science as well as for contemporary students of human evolution. As I believe the present study helps to indicate, for historians paleoanthropology provides a particularly useful field for tracing the relationship between scientific theory and its cultural context. Just as important, it provides a wealth of case studies about the ways in which both general biological theories and more narrow hypotheses within a discipline influence the weighing and interpretation of "empirical" data.

For current practitioners of evolutionary biology the body of "hard" evidence on human evolution has of course expanded tremendously, and methods have become a great deal more sophisticated. Nevertheless, to see how the processes mentioned above have operated in the past cannot help but sharpen their understanding of their own style of reasoning. In addition, if continuities do indeed exist in patterns of interpretation, gaining a deeper understanding of the roots of certain traditional ideas can have a beneficial effect. As William K. Gregory pointed out at the outset of his long and enviably productive career, the practice of examining again and again "one's basal assumptions" is one that a scientist, or any other intellectual for that matter, can ignore only at his or her peril.

NOTES

Introduction

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2. A comprehensive listing of these finds with bibliography on each is contained in Michael H. Day, <u>Guide to Fossil Man: A Handbook of Human Paleontology,</u> 3rd. edition (Chicago: University of Chicago Press, 1977).

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4. Thomas S. Kuhn, <u>The Structure of Scientific Revolutions,</u> 2nd. edition (Chicago: University of Chicago Press, 1970), pp. 10-51. On typology see Ernst Mayr, "The Nature of the Darwinian Revolution," pp. 277-296 in Mayr, <u>Evolution and the Diversity of Life</u> (Cambridge: Harvard University Press, 1976); on the development of the archeological paradigm by the French school of prehistorians, see Michael Hammond, "Anthropology as a Weapon of Social Combat in Late Nineteenth Century France," <u>Journal of the History of the Behavioral Sciences</u> 16 (1980): 118-132, and Donald K. Grayson, <u>The Establishment of Human Antiquity</u> (New York: Academic Press, 1983).

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6. Eldredge and Tattersall, <u>Myths of Human Evolution;</u> Misia Landau, "Human Evolution as Narrative," <u>American Scientist</u> 72 (1983): 262-268; Matt Cartmill, "'Four Legs Good, Two Legs Bad:' Man's Place (if any) in

Nature," _Natural History,_ November 1983: 65-78.

7. See for example Noel T. Boaz, "History of American Paleoanthropological Research on Early Hominidae, 1925-1980," _American Journal of Physical Anthropology_ 56 (1981): 397-405; Fred H. Smith and Frank Spencer, "The Significance of Aleš Hrdlička's 'Neanderthal Phase of Man," _American Journal of Physical Anthropology_ 56 (1981): 435-459; Michael Hammond, "The Expulsion of the Neanderthals from Human Ancestry: Marcellin Boule and the Social Context of Scientific Research," _Social Studies of Science_ 12 (1982): 1-36; Charles Loring Brace, "Tales of the Phylogenetic Woods: the Evolution and Significance of Phylogenetic Trees," _American Journal of Physical Anthropology_ 56 (1981): 411-429; and James C. Fleagle and William L. Jungers, "Fifty Years of Higher Primate Phylogeny," pp. 187-230 in Frank Spencer ed., _A History of American Physical Anthropology, 1930-1980_ (New York: Academic Press, 1982).

8. See for example, Ruse, _Darwinism Defended,_ pp. 130-142 and Peter J. Bowler, _Evolution: the History of an Idea_ (Berkeley: University of California Press, 1984), pp. 329-334 for discussion of these issues.

9. Landau, "Evolution as Narrative," p. 263.

10. Cartmill, for example, makes a strong case that pessimism about human nature conditioned by the horrors of World War II was a critical factor underlying the "killer ape" and "man the hunter" scenarios of hominid evolution that were popular in the 1950s and early 1960s. See Cartmill, "'Four Legs Good,'" pp. 74-76.

11. See for example, Noel T. Boaz, "Research on Early Hominidae;" Frank Spencer,"The Neanderthals and Their Evolutionary Significance: A Brief Historical Survey," pp. 1-49 in Fred H. Smith and Frank Spencer eds., _The Origins of Modern Humans: A World Survey of the Fossil Evidence_ (New York: Alan R. Liss, 1984); Smith and Spencer, "Hrdlička's 'Neanderthal Phase of Man;'" Hammond, "The Expulsion of the Neanderthals;" Charles Loring Brace, "The Fate of the 'Classic' Neanderthals: a Consideration of Hominid Catastrophism," _Current Anthropology_ 5 (1964): 3-46; and Erik Trinkaus, "A History of _Homo Erectus_ and _Homo Sapiens_ Paleontology in America," pp. 261-280 in Frank Spencer ed., _A History of American Physical Anthropology, 1930-1980_ .

12. See the essays in Ernst Mayr and William

Provine eds., The Evolutionary Synthesis: Perspectives on the Unification of Biology (Cambridge: Harvard University Press, 1980); and Edward E. Hunt, "The Old Physical Anthropology," American Journal of Physical Anthropology 56 (1981): 339-346, on the slow absorption of the new methods in the discipline. Peter J. Bowler, The Idea of Evolution, gives a brief account of the development of the "new synthesis", and William Provine, The Origins of Theoretical Population Genetics (Chicago: University of Chicago Press, 1971), provides an analysis of the discipline that was central to the neo-Darwinian perspective.

13. Examples of those others whose contributions will be introduced in what follows are the comparative anatomists Gerrit S. Miller (1869-1956), James H. McGregor, and Dudley J. Morton, the primatologists Adolph H. Schultz (1891-1976) and William L. Straus, Jr. (1900-1981), and the paleoanthropologist Theodore McCown (1908-1969).

14. See the essays in Regna Darnell ed., Readings in the History of Anthropology (New York: Harper and Row, 1974), and also Darnell, "The Development of American Anthropology, 1879-1920: from the Bureau of American Ethnology to Franz Boas" (Ph.D. dissertation, University of Pennsylvania, 1970).

15. Frank Spencer, "The Rise of Academic Physical Anthropology in the United States, 1880-1980: a Historical Overview," American Journal of Physical Anthropology 56 (1981): 353-364.

16. It is interesting to note that the physical anthropologist who actually took the next step, to wonder whether the definition of types really was a useful exercise was Boas. See George J. Armelagos, David S. Carlson And Dennis P. Van Gerven, "The Theoretical Foundations and Development of Skeletal Biology," pp. 306-320 in Frank Spencer ed., A History of American Physical Anthropology, for a discussion of the typological style and its effects on a major division of the discipline.

17. The seminal writings in which Elliot Smith established his authority in the interpretation of the primate brain are "The Arris and Gale Lectures on Some Problems Relating to the Evolution of the Brain," Lancet n.s. 1 (1910): 1-6, 146-153, 221-227, and especially "Address to the Anthropological Section (The Evolution of

Man)," *Report of the British Association for the Advancement of Science, Section H* (1912): 575-598.

18. See for example Stephen Jay Gould, *The Mismeasure of Man* (New York: W.W. Norton, 1981); John S. Haller, Jr., *Outcasts from Evolution: Scientific Attitudes of Racial Inferiority, 1859-1900* (Urbana: University of Illinois Press, 1971); and Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800-1960* (London:Macmillan, 1982).

19. Peter J. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades Around 1900* (Baltimore: Johns Hopkins University Press, 1983) discusses Osborn's influence; on Boule's role in spreading these theories see Hammond, "The Expulsion of the Neanderthals."

20. Wallace's "defection" from Darwinism is discussed in R. Smith, "Alfred Russell Wallace: Philosophy of Nature and Man, *British Journal of the History of Science* 6 (1972): 177-199, and Malcolm Jay Kottler, "Alfred Russell Wallace, the Origin of Man, and Spiritualism," *Isis* 65 (1974): 145-192.

21. Elliot Smith, "Address to the Anthropological Section," provides the classic statement of the "brain first" theory; Michael Hammond, "A Framework of Plausibility for an Anthropological Forgery: the Piltdown Case," *Anthropology* 3 (1979): pp. 47-58, notes how popular this view was in the years around World War I. Hooton discussed his reservations about the "arboreal theory" in "Doubts and Suspicions concerning Certain Functional Theories of Primate Evolution," *Human Biology* 2 (1930): 223-249.

22. Boaz, "Research on Early Hominidae," discusses this point.

23. Elliot Smith, *The Evolution of Man: Essays* (Oxford: Oxford University Press, 1924) helped spread this usage, but Arthur Keith, *The Antiquity of Man* (London: Williams and Norgate, 1915) was perhaps more influential in disseminating the early Pleistocene *Homo sapiens* theory that the terminology referred to.

24. The phrase is from Hrdlička, "The Neanderthal Phase of Man," *Journal of the Royal Anthropological Institute of Great Britain* 57 (1927): 249-274, p. 270; Hammond, "A Framework of Plausibility," stresses the

existence of this favorable theoretical climate for the Piltdown "fossils."

25. See John C. Fleagle And William L. Jungers, "Fifty Years of Higher Primate Phylogeny," for an analysis of this issue.

26. T.D. Stewart, "The Development of the Concept of Morphological Dating in Connection with Early Man in America," Southwestern Journal of Anthropology 5 (1949): 1-16; and Fred H. Smith, "On the Application of Morphological "Dating" to the Hominid Fossil Record, Journal of Anthropological Research 33 (1977): 303-316.

Chapter I

1. William K. Gregory, "Henry Fairfield Osborn, 1857-1935," National Academy of Sciences: Biographical Memoirs 19 (1938): 53-119, p.53. Gregory's perceptive, yet affectionate memoir is the best single source on Osborn's life and work. There is a brief, and appreciative assessment of Osborn by the eminent paleontologist George Gaylord Simpson in the Dictionary of American Biography, First Supplement. Osborn's own scientific autobiography, though somewhat pompous, should also be consulted -- Henry Fairfield Osborn, Fifty-Two Years of Research, Observation and Publication (New York: Scribner's, 1930). Both the Gregory and Osborn works contain extensive bibliographies of the latter's writings.

2. Gregory, "Henry Fairfield Osborn," pp. 54-55.

3. Paul F. Boller, Jr., American Thought in Transition: the Impact of Evolutionary Naturalism (Chicago: Rand McNally, 1969) pp. 29-31.

4. Gregory, "Henry Fairfield Osborn," p. 56.

5. Gregory, "Henry Fairfield Osborn," pp. 59-60; see also Gregory, "Obituary: Henry Fairfield Osborn," Proceedings of the American Philosophical Society 76 (1936): 395-408, pp. 396-397, and Gregory, "A Half Century of Trituberculy. The Cope-Osborn Theory of Dental Evolution, with a Revised Summary of Molar Evolution from Fish to Man," Proceedings of the American Philosophical Society 73 (1934): 169-317.

6. Gregory, "Henry Fairfield Osborn," pp. 70-72; a recent article which discusses Osborn's institutional role at both Columbia and the American Museum around the turn of the century is Douglas Sloan, "Science in New York City, 1867-1907," Isis 71 (1980): 35-76.

7. Gregory, "Henry Fairfield Osborn," p. 73-74.

8. Ibid, p. 73.

9. "Obituary: Henry Fairfield Osborn," New York Times. November 7, 1935, p. 23. For a recognition of the importance of the American Museum in modernizing exhibit styles and increasing public access to fossil collections, see Joseph T. Gregory, "North American Vertebrate Paleontology, 1776-1976," pp. 305-335 in C.J. Schneer ed., Two Hundred Years of Geology in America (Hanover: University Press of New England, 1975), p. 317.

10. "Obituary: Henry Fairfield Osborn," New York Times . For a description of the Hall of the Age of Man see Osborn, The Hall of the Age of Man in the American Museum. Guide Leaflet no. 52 (New York: American Museum of Natural History, 1920), and subsequent editions.

11. Osborn, Fifty-Two Years, passim.

12. Ibid., p.56.

13. Gregory, "Henry Fairfield Osborn," p. 73.

14. Ibid., pp. 73-75.

15. I am thinking here of works like Osborn, Impressions of the Great Naturalists (New York: Scribner's, 1924), and Edward Drinker Cope: Master Naturalist (Princeton: Princeton University Press, 1931).

16. Osborn, Impressions, p. 114.

17. In regard to the immigration issue, see John Higham, Strangers in the Land: Patterns of American Nativism, 1960-1925 (New York: Atheneum, 1970), p. 274; Allan Chase, The Legacy of Malthus (New York: Knopf, 1977), pp. 165-166, 277-279; and Kenneth Ludmerer, Genetics and American Society (Baltimore: Johns Hopkins University Press, 1972), pp. 87-89. On the Scopes trial see Ray Ginger, Six Days or Forever: Tennessee vs. John Scopes (Boston: Beacon Press, 1958), pp. 76, 81, 87; and

also L. Sprague DeCamp, The Great Monkey Trial (Garden City: Doubleday, 1972), pp. 93-96.

18. See for example, Osborn, "Preface," in Madison Grant, The Passing of the Great Race (New York: Scribner's, 1916); Osborn, "Address of Welcome to the Second International Congress of Eugenics," Science n.s. 54 (1921): 311-313; Osborn, "Race Progress in Relation to Social Progress," Journal of the National Institute of Social Science 9 (1924): 8-18. Higham has, of course, ably revealed the role that "scientific" racism played in the eugenics and immigration restriction crazes of the early 20th century and described its main features, especially in Strangers in the Land, pp. 131-157 and 270-277. That Osborn shared in the racist viewpoint fully is entertainingly obvious in one letter he wrote to the New York Times during the immigration debate. In that letter he tried to clear up what he felt was the common confusion between race and nationality that opponents of restriction often made. One must be able to distinguish, Osborn asserted, between gifted individuals of Nordic background in a national group and the great mass of racially inferior stock these individuals lived among. Just because "Nordics" like Dante, Columbus and Galileo had lived in Italy, or Pulaski and Kosciusko in Poland, did not make Italians or Poles as a group a good risk for admission to America. That a way could always be found to declare an exceptional individual "Nordic" was Osborn's typically racist assumption. See Osborn, "Letter to the Editor," New York Times, April 8, 1924, 18.

19. Osborn Fifty-Two Years, p. 5.

20. The major article in this connection is a lecture he gave in 1892, "The Contemporary Evolution of Man," American Naturalist 26 (1892): 455-481, which mainly deals with the ways in which various races of man show different stages of progress in the evolution of key human characters. As one might expect the white race is the most advanced in all major categories. The style of argument is similar to the evolutionary racism of E.D. Cope, "The Developmental Significance of Human Physiognomy," in his Origin of the Fittest: Essays on Evolution (New York: Appleton & Co., 1887), pp. 281-293.

21. Osborn first enunciated this principle in 1902; see "The Law of Adaptive Radiation," American Naturalist 36 (1902): 353-363.

22. Osborn, The Age of Mammals (New York:

Macmillan, 1910), pp. 29-32.

23. Ibid.

24. Ibid., pp. 32-34. Osborn first developed this idea in 1902. See Osborn, "Homoplasy as a Law of Latent or Potential Homology," American Naturalist 36 (1902): 259-271.

25. Osborn, The Age of Mammals, pp. 34-35.

26. Osborn developed his view of the question in relation to the ancestry of man in the following articles of the late 1920s: Osborn, "Recent Discoveries Relating to the Origin and Antiquity of Man," Science, n.s. 65 (1927): 481-488, also in Proceedings of the American Philosophical Society 66 (1927): 373-389; "The Influence of Bodily Locomotion in Separating Man from the Monkeys and Apes," Scientific Monthly 26 (1928): 385-389; and "Is the Ape-Man a Myth," Human Biology 1 (1929): 4-9. Osborn's principal opponent on this issue and on others relating to the problem of man's degree of kinship to the apes was William K. Gregory. See especially Gregory, "Were the Ancestors of Man Primitive Brachiators?", Proceedings of the American Philosophical Society 67 (1928): 129-150; "The Upright Posture of Man: A Review of its Origin and Evolution," ibid., 339-376; "Is the Pro-Dawn Man a Myth?," Human Biology 1 (1929): 153-165; and "A Critique of Professor Osborn's theory of Human Origin," American Journal of Physical Anthropology 14 (1930): 133-161.

27. Osborn, The Age of Mammals, p. 79. Again, the statement of this principle in this synthetic work did not mark its first appearance in Osborn's writings. The "Holarctic" theory, along with a map illustrating its specific application appeared first in 1900. See Osborn, "The Geological and Faunal Relations of Europe and America during the Tertiary Period," Science n.s. 11 (1900): 561-574.

28. The classic statement of the revised "Holarctic" theory, in fact, came from Matthew: W.D. Matthew, "Climate and Evolution," Annals of the New York Academy of Science 24 (1915): 171-318. It, and Osborn's attempt to apply it to hominid evolution will be discussed below.

29. Osborn, The Age of Mammals, p. 80.

30. Ibid., pp. 203-204.

31. Ibid., pp. 381-384.

32. Ibid., p. 384.

33. Ibid. In judging the relative worth of various "eolithic" industries' claim to authenticity, Osborn followed an American archeologist, George Grant MacCurdy of Yale, who had published an exhaustive survey of the problem in 1905. See MacCurdy, "The Eolithic Problem -- Evidences of a Rude Industry Antedating the Paleolithic," American Anthropologist, n.s. 7 (1905): 425-479. The "eolithic" question was much debated in the first quarter of this century; the general trend since around 1910 has been to dismiss "eoliths" as cases of natural fracturing of flint, but, as we shall see below, there were some major exceptions to the trend, at least temporaarily. The general issue of "eoliths" will also be looked at later in the context of MacCurdy's work.

34. Osborn, The Age of Mammals, pp. 376-377. The four glaciation theory of the Pleistocene in Europe, after an initial period of debate, became an accepted convention in paleoanthropology until after WW II. On its establishment, see Kenneth P. Oakley, "The Problem of Man's Antiquity: A Historical Survey," Bulletin of the British Museum (Natural History). Geological Series, v. 9, no. 5. (1964), pp. 129-131.

35. Osborn, The Age of Mammals, pp. 378-379; on the Abbevillian, see Oakley, "The Problem of Man's Antiquity," p. 127.

36. What I mean by "recognizable" culture is meant to exclude "eoliths" for the following reason -- if culture includes mainly learned and transmittable patterns of behavior, only a tool that is formed after a standard pattern, however crude, can be identified as an element of culture. As we shall see in our discussion of MacCurdy, eoliths were generally believed to have marks of human use on them, but no standard pattern.

37. Osborn, The Age of Mammals, p. 381. Glyn Daniel, in one of his works on the history of archaeology, has noted the wide influence of this scheme of unilinear "epochs," and the fact that its founder, the Frenchman Gabriel de Mortillet, consciously modelled it on geology. That it would be attractive to natural scientists like Osobrn was thus not surprising. Unfortunately, it did not fit well with other assumptions that they often brought to

their analyses of human evolution, as we shall see below. On the de Mortillet scheme and its eventual disintegrtion, consult Glyn Daniel, A Hundred Years of Archaeology (London: Duckworth, 1950), pp. 106-108, 123-126, 231-232, 244, and Michael Hammond, "Anthropology as a Weapon of Combat in Late 19th Century France."

38. Osborn, The Age of Mammals, pp. 385,404. The story of the discovery of each of these fossils, which are now classed as specimens of Homo erectus , can be found, among other places, in Herbert Wendt, In Search of Adam: The Story of Man's Quest for Truth abouthis Earliest Ancestors, trans. from the German by Janes Clough (Boston: Houghton Mifflin, 1956), pp. 290-301, 398-405.

39. Osborn, The Age of Mammals, pp. 410-412. The literature on the Neanderthals is of course immense, and the finer points raised in it are both beyond the scope of this essay and beyond the expertise of its author. A general intorudction to the Neanderthal problem can be obtained from C.L. Brace and Ashley Montagu, Human Evolution (New York: Macmillan, 1977); W.W. Howells, The Evolution of the Genus Homo (Reading, Mass.: Addison-Wesley, 1973); Kenneth A.R. Kennedy, Neanderthal Man (Minneapolis: Burgess, 1975); and Erik Trinkaus and William W. Howells, "The Neanderthals," Scientific American, 241 (1979): 118-133. A standard general introduction to the Mousterian, a tool tradition long associated with the Neanderthals of Western Europe, is contained in Francois Bordes, The Old Stone Age (New York: McGraw Hill, 1968), pp. 98-120. A recent, comprehensive look at the issue is contained in the essays collected in Fred H. Smith and Frank Spencer, The Origins of Modern Humans: A World Survey of the Fossil Evidence (New York: Alan R. Liss, 1984).

40. Osborn, The Age of Mammals, p. 411.

41. Osborn, "Men of the Old Stone Age," American Museum Journal 12 (1912): 279-295. The great impact that cave art made on Osborn was also not an unusual event in the period under consideration. Though wall paintings had first been discovered in the caves of northern Spain the the late 1870s, general acceptance of the idea that they were artifacts of Upper Paleolithic cultures did not come until the period between 1900 and 1910. See Daniel, A Hundred Years of Archaeology, pp. 130-132.

42. Osborn, "Men of the Old Stone Age," p. 282. Breuil was of course a crucial figure in the development

of prehistoric archeology. Daniel notes that in the long run Breuil's work undermined the unilinear de Mortillet scheme, and helped establish the notion that Paleolithic tool types defined "cultures" rather than "epochs" of cultural evolution. In the short term, however, Breuil's work on the Upper Paleolithic constituted a refinement of the traditional evolutionary appraoch, and not an attack on it. See Glyn Daniel, A Hundred Years of Archaeology, pp. 231-232; and The Idea of Prehistory (Cleveland: World, 1963), 97-101. Breuil's most important theoretical paper of the pre- World War I period was Henri Breuil, '"Les Subdivisions du Paleolithique Superior et leur Signification," Comptes Rendus de la Congrès International d'Anthropologie et d'Archéologie Préhistorique, Geneva, 1912, pp. 165-239. See also Alan H. Broderick, Father of Prehistory: The Abbé Henri Breuil (New York: Morrow, 1963), 53-55. Nothing could highlight the diverging paths that cultural and biological anthropologists would be taking in the years ahead more than the "cultural" analysis of Upper Paleolithic art that the American Museum's Clark Wissler appended to Osborn's article. Rather than focusing on the unique racial qualities of those who made the art, Wissler asserted that the complexity of expression, wide geographic distribution and long period of development of cave art

> make it clear that the cultural view of modern man applies equally well to the man of antiquity and that we are quite right in interpreting Aurignacian culture by what we know of living races ... their artists must have accomplished their work just as we would and moved along in the construction of their culture by steps analogous to our own. In other words, the universal human was there in that dim remote past, as it is with us still [emphasis mine].

In order to cement this point, Wissler further cautioned against attributing the idea of "natural evolution" to the development of Upper Paleolithic art, since art is a "psychological phenomeneon not a biological one." See remarks by Wissler in Osborn,"Men of the Old Stone Age," pp. 292-293. For historical views on this divergence see G.W. Stocking, "Ideas and Institutions in American Anthropology: Thoughts Toward a History of the Interwar Years," in Selected Papers from the American Anthropologist, 1921-1945 (Washington: American Anthropologial Association, 1967); and especially Hamilton Cravens, "American Scientists and the Heredity-Environment Controversy, 1883-1940" (Ph.D. Dissertation, University of Iowa, 1969, pp. 124-257.

43. Osborn, Men of the Old Stone Age: Their Environment, Life and Art (New York: Scribner's, 1915). J.C. Merriam, "Review of Men of the Old Stone Age, by Henry Fairfield Osborn," American Anthropologist, n.s. 18 (1916): 426-429, p. 426.

44. The person most responsible for defining the problems of "typological thinking" is the biologist Ernst Mayr. Mayr believes that the "typologist" represents a philosophical attitude descended from Platonic "essentialism" that is diametrically opposed to the approach required by modern population biology. See Mayr, Populations, Species and Evolution (Cambridge; Harvard, 1970), pp. 4-5, as well as several of the essays in Mayr, Evolution and the Diversity of Life (Cambridge: Harvard University Press, 1976).

45. Boule's immensely influential monograph appeared in 1912 and 1913; Marcellin Boule, "L'Homme Fossile de la Chapelle-aux-Saints," Annales de Paléontologie 7 (1912): 85-192, 8 (1913): 1-67. Modern critical analysis of its influence on the interpretation of fossil man began with C.L. Brace, "The Fate of the 'classic' Neanderthals: A Consideration of Hominid Catastrophism," Current Anthropology 5 (1964): 3-46. More recent discussions are contained in Hammond, "The Expulsion of the Neanderthals," and Spencer, "The Neanderthals and Their Envolutionary Significance."

46. See, for example, Boule, "L'Homme Fossile de La Chapelle-aux-Saints," and especially the writings of Sir Arthur Keith, because of the latter's great influence in the English speaking world. Keith, The Antiquity of Man (London: Williams and Norgate, 1915), lays out his polyphyletic theory in detail, while Keith, An Autobiography (London: Watts, 1950), pp. 318-330, 347, gives an account of his conversion.

47. This particular problem was the one that Ales Hrdlicka, the principal American opponent of polyphyletism, tended to stress in his defense of the Neanderthal "phase" of man. See the essay on Hrdlicka below, pp. 277-279. For more detailed discussion of the scheme of Paleolithic cultural evolution, see the chapter on MacCurdy, pp. 172-178.

48. Osborn, Men of the Old Stone Age, pp. 77-79. This phenomenon has been noted again and again in works on human evolution written since World War II; that it was nearly universal in the years between the wars was first

pointed out by an American sociologist, J.M. Gillette, "Ancestorless Man: The Anthropological Dilemma," Scientific Monthly 57 (1943): 533-545.

49. Osborn, Men of the Old Stone Age, p. 96.

50. Ibid., p. 140. Elliot Smith's original statement of this thesis is in an appendix to Charles Dawson and Arthur Smith Woodward, "On the Discovery of a Paleolithic Human Skull and Mandible in a Flint Bearing Gravel overlying the Wealden (Hastings Beds) at Piltdown, Sussex," Quarterly Journal of the Geological Society of London 69 (113): 117-144.

51. Osborn, Men of the Old Stone Age, p. 144.

52. Ibid.

53. Ibid., p. 78. The frontal region of the brain was generally considered at the time to be the seat of higher mental activities by students of the brain, and students of fossil man claimed to be able to gauge relative intelligence of their specimens by examining the markings on the interior of the skull. The latter would appear, more or less clearly, if a cast were taken of the interior of the skull vault. The diagnostic value of the "endocast" thus obtained depended, of course,on two subsidiary assumptions -- a) that the markings on the skull matched the foldings on the living brain, and b) that the folding, or convolutional, complexity of the brain revealed the degree of complexity of the mental functioning of an animal. The dubiousness of both assumptions eventually caused the abandonment of qualitative studies of the andocast. The literature on endocasts and their interpretation is immense; a good introduction to recent thinking on the subject is Harry J. Jerison, "Fossil Evidence of the Evolution of the Human Brain," Annual Review of Anthropology 4 (1975): 27-58.

54. Osborn, Men of the Old Stone Age, p. 83.

55. Osborn, Men of the Old Stone Age, pp. 83-84. This point is significant also because it would contrast greatly with his later judgement on the antiquity of man. In 1915, even though he did not accept the primitive Java specimen as a human ancestor, he was in favor of a relatively recent date for a Pithecanthropus -like stage. In a more technical article of 1915 which weighed the opinions of various authorities on prehistory, Osborn highlighted the reasoning behind this view when he

asserted that "the trend of Paleolithic research lately has been to draw all the human culture periods... closer together and reduce the time assigned for their evolution." This fact resulted from conservative dating of early tool traditions like the Chellean, but also from a greater scepticism toward eoliths. The existence of this trend in 1915 makes the reversal of the 1920s more dramatic. See Osborn, "Review of the Pleistocene of Europe, Asia and Northern Africa," Annals of the New York Academy of Science 26 (1915): 215-315.

56. Osborn, Men of the Old Stone Age, pp. 128-129.

57. Ibid., p. 150; see also Osborn, "Review of the Pelistocene," p. 233.

58. Osborn, Men of the Old Stone Age, p. 135. Today, of course, an anthropologist would be very hesitant to date a geological deposit on the basis of a few, or even scores of artifacts. That Osborn was so willing to do so underlines the strong influence that the idea of cultural "epochs" had on him. The riskiness of the procedure becomes especially evident in the context of the Piltdown forgery. The supposedly "Pre-Chellean" artifacts were as phony as the Piltdown skull and jaw fragments. See J.S. Weiner, The Piltdown Forgery (London: Oxford University Press, 1955), pp. 54-69.

59. The present consensus in paleoanthropology associates both of these tool traditions with Homo erectus. See, for example, G.E. Kennedy, Paleoanthropology (New York: McGraw Hill, 1980), pp. 305-349.

60. Osborn, Men of the Old Stone Age, p. 113.

61. Ibid., p. 180.

62. Ibid.

63. Ibid., pp. 270-272.

64. Ibid., p. 277.

65. Ibid., pp. 256-258. See especially Aleš Hrdlička, "The Most Ancient Skeletal Remains of Man," Smithsonian Institution Annual Report for 1913 (1914): 491-552.

66. Osborn, Men of the Old Stone Age, p. 232.

67. This expert influence would probably be especially great in a popular, synthetic work like Men of the Old Stone Age, where one would expect a desire for acceptance of one's efforts by the recognized experts in the field.

68. Osborn, Men of the Old Stone Age, pp. 278-279.

69. Peter J. Bowler, The Eclipse of Darwinism, p. 131. This is not to say that Osborn used his Neo-Lamarckism, as some did, to assert either actual or potential human equality. Like Cope, Osborn felt that great racial differences would be a permanent feature of the human social landscape; see Osborn, "The Contemporary Evolution of Man." On Cope's racist views, see John S. Haller, Outcasts from Evolution: Scientific Attitudes of Racial Inferiority, 1865-1900, (Urbana: University of Illinois Press, 1971), pp. 197-198.

70. Ludmerer, Genetics and American Society, p. 39; Cravens, "Heredity-Environment Controversy," pp. 10-13.

71. Osborn, Men of the Old Stone Age, p. 279.

72. Ibid.

73. Ibid., p. 272.

74. C.L. Brace, "The Fate of the 'Classic' Neanderthals.".

75. Osborn, Men of the Old Stone Age, pp. 271-272.

76. Ibid., p. 450, Peter J. Bowler also discusses the racial "life cycle" idea in The Eclipse of Darwinism .

77. Brace, "The Fate of the 'Classic' Neanderthals." The case of the Neanderthal gait shows as well as any other example the danger of over-reliance on "type" specimens. In the 1950s a re-examination of the La Chapelle-aux-Saints skeleton revealed that the unfortunate "old man" had been suffering from an advanced case of a form of arthritis. The diminished curvature of the spine that Boule had seen as evidence of the Neanderthal "stoop" was a product of the disease, and not a Neanderthal "character." See W.L. Straus Jr. and A.J.E. Cave, Pathology and the Posture of Neanderthal Man," Quarterly Review of Biology 32 (1957): 348-363.

78. Osborn, Men of the Old Stone Age, p. 224.

79. Ibid., p. 237.

80. See Boule and Anthony, "L'Encephale de l'Homme Fossile de La Chapelle-aux-Saints, L'Anthropologie 22 (1911): 194-213. In fact, it was scepticism regarding these conclusions, and those of Elliot Smith on the Piltdown endocast, that lead James Symington to make an experimental study on the relation of endocast details to the detail of actual brains from modern humans. His conclusion, that the endocast was not a good guide to convolutional detail, was widely acknowledged as important, but it did not deter speculation. See Symington, "On the Relations of the Inner Surface of the Cranium to the Cranial Aspect of the Brain," Edinburgh Medical Journal 14 (1915): 85-100, and "Endocranial Casts and Brain Form: A Criticism of Some Recent Speculations," Journal of Anatomy and Physiology 50 (1916): 111-130.

81. Osborn, Men of the Old Stone Age, pp. 58-9, 84.

82. Ibid., p. 316.

83. Ibid., p. 272.

84. Ibid.

85. Ibid., pp. 315-330, 353-354.

86. Ibid., pp. 350-353.

87. Ibid., pp. 298-299.

88. Ibid., p. 316.

89. Higham, Strangers in the Land, pp. 264-299.

90. Osborn, in fact, did make this argument explicitly in 1926. See "The Evolution of Human Races," Natural History 26 (1926): 3-13. The idea of Tertiary man, and the term itself, were not new with Osborn. Both "eoliths" and some apparent instances of human remains of modern form in Tertiary deposits had awakened speculation about Pre-Pleistocene man in the later 19th century. See K.P. Oakley, The Problem of Man's Antiquity, pp. 117-123.

91. See Chase, The Legacy of Malthus, pp. 278.

92. Ibid., p. 274.

93. Osborn, "The Evolution of Human Races," p. 4. The emphasis is Osborn's.

94. Ibid.

95. Quoted in Chase, The Legacy of Malthus, p. 275.

96. Ludmerer, Genetics and American Society, pp. 25-26.

97. For the story of these expeditions, see Harry Chapman Andrews, On the Trail of Ancient Man (New York: Putnam's, 1926), and The Natural History of Central Asia. V. I: the New Conquest of Central Asia (New York: American Museum of Natural History, 1932). The phrase quoted was used in both works; see Osborn, "Preface," p. vii in the former work and in the latter work by Andrews, p. 453.

98. Osborn, " Hesperopithecus : the First Anthropoid Primate Found in America," American Museum Novitiates, no. 37 (New York: American Museum of Natural History, 1922); also see Gregory and Milo Hellman, "Notes on the Type of Hesperopithecus haroldcooki Osborn," American Museum Novitiates, no. 53 (New York: American Museum of Natural History, 1923). The issue is discussed further in the chapter on Gregory. See pp. 594-595 and note 228 on that chapter below.

99. The quote is from Osborn, "The Pliocene Man of Foxhall in East Anglia," Natural History 21 (1921): 565-576, p. 566.

100. The story of Reid Moir's discoveries and a detailed discussion of the "Foxhallian" industries are found in Osborn, Man Rises to Parnassus: Critical Epochs in the Prehistory of Man (Princeton: Princeton Univ. Press, 1928), pp. 23-45. Reid Moir told his own story in James Reid Moir, Pre-Paleolithic Man (Ipswich, 1919), and The Antiquity of Man in East Anglia (London, 1927).

101. Osborn, "The Pliocene Man of Foxhall," p. 566. Daniel notes that Breuil's support of Reid Moir was a key factor in the level of acceptance of the latter attained. See Daniel, One Hundred Years of Archaeology, p. 231. As far as the authenticity of Reid Moir's flints is concerned, more recent opinion seems to be in favor of natural manufacture of all the Red Crag and Norwich Crag "assemblages", and in addition, Oakley adds that these two

deposits are Pleistocene and not Pliocene, as Moir and Osborn believed. See Oakley, The Problem of Man's Antiquity, p. 103, and J.M. Coles and E.S. Higgs, The Archaeology of Early Man (New York: Praeger, 1969), p. 202.

102. Osborn, Man Rises to Parnassus, p. 32.

103. Osborn, "The Pliocene Man of Foxhall," p. 573.

104. Osborn, "Recent Discoveries Relating to the Origin and Antiquity of Man," 66 (1927): 373-389., p. 382. That Osborn was lending his full scientific support to Reid Moir comes through especially strongly in the reworking of late Pliocene and Pleistocene chronology that the former published in 1922. In his new scheme Reid Moir's "Foxhallian" and "Cromerian" (an early to mid-Pleistocene industry) "cultures" are given full-fledged status as "historical periods" in the evolution of human culture. Osborn and Chester Reeds, "Old and New Standards of Pleistocene Division in Relation to the Prehistory of Man in Europe," Bulletin of the American Geological Society 33 (1922): 411-490.

105. Osborn, "The Dawn Man of Piltdown, Sussex," Natural History 21 (1922): 565-570; also, see Osborn, Man Rises to Parnassus, pp. 48-76, for a full discussion of Piltdown's significance for Osborn. The chance to examine the Piltdown remains under the watchful eye of a fellow paleontologist, Smith Woodward, made Osborn into a strong supporter of the alleged association between Piltdown's ape-like jaw and human skull. As with others who were inclined to belief, the finding of the so-called "Piltdown II" fragments in 1915 had rendered that association more probable than it had previously seemed. See Ibid., pp. 48-51.

106. Osborn, "The Discovery of Tertiary Man," Science 71 (1930): 1-7, p. 5.

107. See n. 46 above.

108. J.S. Weiner, The Piltdown Forgery, pp. 54-69.

109. See n. 82 above.

110. In Man Rises to Parnassus, Osborn confronted this problem directly: unless "the Piltdown flint workers" were indeed "of Foxhall age" , he noted, we would know

"nothing of the brain weight and little of the intelligence of the Dawn man who fashioned the flints of the sub-Red Crag and Foxhall." Obviously the need to speak to these issues from morphological evidence was felt strongly enough to make him take chances with the dating of Piltdown. Osborn, Man Rises to Parnassus, p. 35.

111. Osborn, "Recent Discoveries Relating to the Origin and Antiquity of Man," pp. 379-380. Note that this is the same endocast that in 1915 was so primitive and ape-like. Actually, Elliot Smith had not revised his conclusions, and Tilney, while somewhat more favorable to Piltdown Man's capabilities, persisted in seeing the Piltdown "brain" as a transitional form bridging the gap between "Pithecanthropus" and the Neanderthals, much as Osborn had in Men of the Old Stone Age. See Frederick Tilney, The Brain from Ape to Man (New York: Hoeber, 1928), pp. 888-893.

112. Osborn, "The Discovery of Tertiary Man," pp. 3-4.

113. Osborn, "Recent Discoveries Relating to the Origin and Antiquity of Man," p. 381.

114. See G.E. Kennedy, Paleoanthropology (N.Y.: McGraw-Hill, 1980), pp. 326-327.

115. See Herbert Wendt, In Search of Adam, pp. 290-301.

116. Osborn, "The Discovery of Tertiary Man," p. 5.

117. Ibid., p. 4.

118. Ibid., p. 3.

119. Ibid., p. 5.

120. Osborn, "The Pliocene Man of Foxhall," p. 566.

121. See the long list of theoretical articles in Osborn's own "classified bibliography", in Osborn, Fifty-Two Years, pp. 101-104.

122. Osborn, "The Origin of Species as Revealed by Vertebrate Paleontology," Nature 115 (1925): 961-963, p. 963.

123. Osborn, "Orthogenesis as Observed from

Paleontological Evidence beginning in the year 1889," American Naturalist 56 (1922): 134-142, p. 137.

124. Osborn, "The Discovery of Tertiary Man," p. 7.

125. Ibid., p. 6; see also Osborn, "Is the Ape-Man a Myth?," Human Biology 1 (1929): 4-9, p. 7.

126. Osborn, "The Discovery of Tertiary Man," p. 6.

127. Very crudely put, "brachiation" is a means of locomotion in an arboreal environment that involves swinging from branch to branch with the weight of the body suspended from the arms. The types of brachiation exhibited by various primates and the question of whether humans are descended from a brachiator are still the subjects of active research and debate. For a good introduction with a historical dimension see Russell Tuttle, "Darwin's Apes, Dental Apes, and the Descent of Man: Normal Science in Evolutionary Anthropology," Current Anthropology 15 (1974): 389-398. The issue has been reviewed more recently by Fleagle and Jungers in "Fifty Years of Higher Primate Phylogeny." Keith developed his view on these issues in Arthur Keith, "The Extent to Which the Posterior Segments of the Body Have Been Transmuted and Suppressed in the Evolution of Man and Allied Primates," Journal of Anatomy and Physiology 37 (1903): 18-40, "Certain Phases in the Evolution of Man," British Medcal Journal 1 (1912): 734-737, 788-790, and The Antiquity of Man (London: Williams and Norgate, 1915), and defended it especially in "Man's Posture: Its Evolution and Disorders," British Medical Journal 11 (1923): 451-454, 499-502, 545-548, 587-590, 624-626, 669-672.

128. See William K. Gregory, "Studies on the Evolution of the Primates. Part II. Phylogeny of Recent and Extinct Anthropoids with Special Reference to the Origin of Man," Bulletin of the American Museum of Natural History 35 (1916): 258-355.

129. These arguments recurred in all Osborn's writings on man in the years 1927-1930, but they are expresed most clearly in Osborn, "Recent Discoveries Relating to the Origin and Antiquity of Man," Proceedings of the American Philosophical Society 66 (1927): 373-389, also in Science 65 (1927): 481-488; "The Influence of Bodily Locomotion in Separating Man from the Monkeys and Apes," Scientific Monthly 26 (1928): 385-399; and "Is the Ape-Man a Myth?", Human Biology 1 (1929): 4-9.

130. Osborn, "The Influence of Bodily Locomotion," p. 397.

131. Ibid.

132. Osborn, "Recent Discoveries," Science 65 (1927): 481-488, p. 484.

133. Osborn, "Influence of Bodily Locomotion," p. 398.

134. Osborn, "Recent Discoveries," p. 484.

135. This data was contained in Adolph H. Schultz, "Growth Studies on Primates Bearing upon Man's Evolution," American Journal of Physical Anthropology 7 (1924): 149-164.

136. Osborn, "Recent Discoveries," p. 484.

137. The theory or recapituation received exhaustive historical and scientific analysis in Stephen Jay Gould, Ontogeny and Phylogeny (Cambridge: Harvard University Press, 1977).

138. Schultz, "Growth Studies of Primates," p. 149.

139. Osborn, "Recent Discoveries," p. 485.

140. Ibid., see also Osborn, "The Discovery of Tertiary Man," p. 6.

141. Ibid., p. 7.

142. Osborn, The Earth Speaks to Bryan (New York: Scribner's, 1925), p. 55.

143. Bowler, The Eclipse of Darwinism, p. 142.

144. Ibid., p. 120.

145. Adrian Desmond, Archetypes and Ancestors: Paleontology in Victorian London (Chicago: University of Chicago Press, 1982), pp. 95-98.

146. Osborn, "The Discovery of Tertiary Man,", p. 6.

147. Ibid.

148. Osborn, "Recent Discoveries," p. 483.

149. Osborn, "Is the Ape-Man a Myth?," p. 6.

150. Osborn, "The Discovery of Tertiary Man," pp. 2-3.

151. Osborn, "Recent Discoveries," p. 484.

152. Ibid.

153. Osborn, "Influence of Bodily Locomotion," p. 386.

154. Osborn, "Recent Discoveries," p. 483.

155. See E.A. Hooton, "Where Did Man Originate?", Antiquity 1 (1927): 137-150; Aleš Hrdlička, "The Peopling of the Earth," Proceedings of the American Philosophical Society 65 (1926): 150-156; Osborn himself noted the long pedigree of the theory that man originated in Asia, and traced its first scientific statement in the U.S. to a paragraph written by the paleontologist Joseph Leidy in 1857. See Osborn, Man Rises to Parnassus, p. 157. Spencer, "The Neanderthals and Their Evolutionary Significance," p. 10, also notes the importance of the theory in mid-19th century European writings.

156. Osborn, Men of the Old Stone Age, p. 272.

157. W.D. Matthew, "Climate and Evolution," Annals of the New York Academy of Science 24 (1915): 171-318. The citations that follow are taken from the 1939 reprint of the original work published in book form by Columbia University Press.

158. Matthew, Climate and Evolution, p. 7.

159. Ibid.

160. Ibid.

161. Ibid., pp. 3-4.

162. Ibid., p. 7.

163. Ibid., p. 32.

164. Ibid., p. 7.

165. Ibid., pp. 40-45.

166. Osborn, _Man Rises to Parnassus,_ p. 156.

167. Joseph Barrell, "Probable Relation of Climatic Change to the Origin of the Tertiary Ape-Man," _Scientific Monthly_ 4 (1917): 16-26, p. 23-25. See the extended analysis of Gregory's monograph in the chapter on Gregory below.

168. Ibid., p. 18.

169. Ibid., p. 23.

170. Ibid., p. 25.

171. Ibid., p. 23.

172. Ibid., p. 25.

173. Osborn, "Asiatic Expeditions of the American Museum of Natural History," _Nature_ 114 (1924), 504-507, p. 506.

174. Ibid.

175. Osborn, "Present Status of the Problem of Human Ancestry," _Proceedings of the American Philosophical Society_ 67 (1928): 151-155, p. 153.

176. Osborn, "Recent Discoveries," p. 487; the "Neanderthaloid races" are also discussed, with more specific description of the fossil and cultural evidence involved, in Osborn, _Man Rises to Parnassus,_ pp. 77-100.

177. Osborn, "Recent Discoveries," p. 487.

178. Ibid.

179. Ibid.

180. Ibid.

181. Ibid.

182. Ibid.

183. Ibid.

184. Ibid. The strong implication oof Negro inferiority in intelligence contained in this analogy was entirely characteristic of Osborn. When writing about modern races he stated his belief explicitly. See Osborn, "The Evolution of Human Races," Natural History 26 (1926): 3-13, p. 5. By ruling out the central African forests as the birthplace of mankind Osborn appeared to be attempting to dispose of the entire continent as well. Only once in his writings of the late 1920s did he acknowledge that Africa contained uplands and savannahs that might provide an environment similar to the one he hypothesized for central Asia; see Osborn, "Is the Ape-Man a Myth?," p. 9. In addition, Osborn never stopped to analyze the fossil, found in 1925, that would do the most in redirecting anthropological attention toward Africa -- *Australopithecus* .

185. Osborn, "Recent Discoveries," p. 487.

186. Ibid., pp. 487-488.

187. Ibid., p. 488.

188. By this time, however, he had come to admit the possibility that the "Cro-Magnons" *might not* be racially distinct from other Upper Paleolithic Europeans, and thus that the term "Cro-Magnon" might have to include the latter as well. See Osborn, *Man Rises to Parnassus,* p. 84.

189. Osborn, "Recent Discoveries," p. 483.

190. Ibid. As far as I can determine, he took "convergence" to be synonymous with the concept of "analogous evolution" that he discussed in 1910. See pp. 42-43 above.

191. Between 1927 and 1929, three articles on Osborn's theoretical views appeared in New York newspapers, and between 1923 and 1926, three more on the central Asian theory of human origin. See the "Bibliography" appended to Osborn, *Fifty-Two Years of Research,* pp. 99-100. In addition to the articles by Hooton and Hrdlička, Osborn's ideas were subjected to extensive criticism by W.K. Gregory, which will receive detailed discussion in Chapter V.

192. The story is told, through 1925, in a popular work by Roy Chapman Andrews, *On the Trail of Ancient Man,* and in more detail in Andrews, *Natural History of Central*

Asia V.I: The New Conquest of Central Asia (New York: American Museum of Natural History, 1932), which also contains summaries of geological and archeological findings by the expeditions' specialists.

193. Andrews, Natural History of Central Asia , pp. 445-446.

194. Schultz, "Growth Studies on Primates," pp. 153-154, 161-162.

195. See, for example, Morton, "Evolution of the Human Foot.I," American Journal of Physical Anthropology 5 (1922): 305-336, and "Evolution of the Human Foot. II," Ibid. 7 (1924): 1-52.

196. See Gregory, "The Origin of Man from the Anthropoid Stem -- When and Where?," Proceedings of the American Philosophical Society 66 (1927): 439-463, and "Were the Ancestors of Man Primitive Brachiators?," Ibid. 67 (1928): 129-150.

197. This impression came through most strongly in Robert M. Yerkes, The Great Apes: A Study of Anthropoid Life (New Haven: Yale University Press, 1929), and Wolfgang Köhler, The Mentality of Apes, trans. by Ella Winter (New York: Harcourt Brace, 1925), both classics in the psychological literature.

198. Hooton,"Where Did Man Originate?"; Hrdlicka, "The Peopling of the Earth."

199. Osborn, "Asiatic Expeditions," p. 506.

200. Aleš Hrdlička, "The Neanderthal Phase of Man," Journal of the Royal Anthropological Institute of Great Britain 57 (1927): 249-275, pp.. 269-270.

201. Osborn, "Recent Discoveries," p. 487.

202. See Grafton Elliot Smith, "Address to the Anthropological Section (H)," Report of the British Association for the Advancement of Science for the Year 1912, pp. 575-598; also Smith, The Evolution of Man: Essays (London: Oxford University Press, 1924).

203. This was an assumption that those who followed Elliot Smith would not accept without reservations, since he believed that the enlarged brain of early hominids was a major cause of the transition to ground dwelling. See

Elliot Smith, "Address to the Anthropological Section," p. 595.

204. For a discussion of late 19th century evolutionary racism consult John S. Haller, Jr., Outcasts from Evolution, passim.

205. Ellsworth Huntington, World Power and Evolution (New Haven: Yale University Press, 1919), p. 142, and Civilization and Climate (New Haven: Yale University Press, 1915), passim .

206. Huntington, Civilization and Climate, pp. 174-181.

207. Matthew, "Climate and Evolution," pp. 40-45.

208. Osborn, "The Evolution of Human Races," p. 6. It should also be noted that the phrase "organic selection" in the passage above is not equivalent to natural selection. Rather, Osborn was referring to the so-called Baldwin effect (of which Osborn considered himself a co-discoverer), a principle of evolution which, its defenders felt, provided a "third way" between Darwinian selection of chance variation and Lamarckian inheritance of acquired characteristics. Modern orthodoxy finds "organic selection" a superfluous concept -- what is valid within it can be easily understood as a special case of Darwinian selection. See Ernst Mayr, Animal Species and Evolution (Cambridge: Harvard University Press, 1963), pp. 610-612. Baldwin had elaborated his view of "Organic Selection" in James Mark Baldwin, Development and Evolution (New York, 1902).

209. Osborn, "Evolution and Human Races," pp. 4,6.

210. This point is amply demonstrated in Stephen Jay Gould's recent work on biological attempts to determine levels of intelligence in humans, The Mismeasure of Man (New York: Norton, 1981).

211. W.K. Gregory, "Is the Pro-Dawn Man a Myth?," Human Biology 1 (1929): 153-165. Indeed, Gregory's belief that evolution by changes in proportion, which Osborn dubbed "allometrons," could reverse direction, caused the former to dispute his mentor's picture of the phylogeny of the titanotheres, one of the groups Osborn had studied most intensively during his career. See Gregory's essay, "Summary of Harmonic and Differential Allometrons in the Skulls and Feet and an Interpretation of the Phylogeny of

the Titanotheres," pp. 828-833 in Osborn, Titanotheres of Ancient Wyoming, Dakota and Nebraska . U.S. Geological Soceity Monograph No. 55, v.2 (1929).

212. For Gregory, see n. 173 and n. 188 above; for Hooton, see Hooton, "Where did Man Originate?", and especially his Up From the Ape (New York: Macmillan, 1931).

213. In America, this scepticism was chiefly represetned by Schultz, who supported a close relationship between hominids and apes, but was hesitant to follow Gregory in hypothesizing a dryopithecine common ancestor for man and the chimpanzee-gorilla stock; and by W.L. Straus, Jr., a distinguished student of Schultz, who doubted that man's ancestors had passed through an ape-like stage at all. See Fleagle and Jungers, "Fifty-Years of Higher Primate Phylogeny," as well as Adolph Schultz, "Characters Common to Higher Primates and Characters Specific for Man," Quarterly Review of Biology 11 (1936): 259-283, 425-455; and W. L. Straus, Jr., "The Riddle of Man's Ancestry," Quarterly Review of Biology 24 (1949): 200-223.

214. For Black's role in the discoveries see Harry L. Shapiro, Peking Man (New York: Simon and Schuster, 1974), pp. 38-52, and Dora Hood, Davidson Black: A Biography (Totonto: University of Toronto Press, 1964).

215. Davisdon Black, "Asia and the Dispersal of the Primates," Bulletin of the Geological Society of China 4 (1925): 133-183.

216. Davidson Black, "On the Discovery, Morphology and Environment of Sinanthropus Pekinensis ," Philosophical Transactions of the Royal Society of London, Ser. B, 223 (1934): 57-120.

217. Osborn, "The Discovery of Tertiary Man," p. 2.

218. Hooton, Up From the Ape , p. 325.

219. Franz Weidenreich, "The Skull of Sinanthropus Pekinensis : A Comparative Study on Primitive Hominid Skull," Paleontologia Sinica, New Ser. D, no. 10, Whole Series 127 (1943). Respect, of course, does not equal acceptance of Weidenreich's position, which was monophyletic in that it placed most forms of fossil humanity back on the line of evolution leading to Homo sapiens, but polyphyletic in the way it allowed various

racial lines to evolve _in situ_ from the _erectus_ to the _sapiens_ stage of development. This amalgam proved to be unacceptable to most paleoanthropologists. In the 1940s it was the monophyletic aspect of the theory that casued the most controversy. E.A. Hooton, "Review of Weidenreich, The Skull of _Sinanthropus Pekinensis_ ," _American Journal of Physical Anthropology_ n.s. v. 2 (1944): 318-319, and W.W. Howells, "Fossil Man and the Origin of Races," _American Anthropologist_ n.s. 44 (1942): 182-193.

220. Hooton, "Where did Man Originate;" Shapiro, _Peking Man,_ p. 23.

221. This criticism of earlier work is most strongly expressed in the writings of Sherwood L. Washburn. See for example Washburn, "The Strategy of Physical Anthropology," in A.L. Kroeber ed., _Anthropology Today_ (Chicago: University of Chicago Press, 1953).

222. This interest in general questions of evolutionary theory and their application marked Osborn's paleontological work as well, at a time when most paleontologists tended to favor a dead-ended "empirical" approach. See G.G. Simpson, "Henry Fairfield Osborn," _Dictionary of American Biography_ (New York: Scribner's, 1944), 1st supp., 584; and Stephen Jay Gould, "G.G. Simpson, Paleontology, and the Modern Synthesis," pp. 153-172 in E. Mayr and W.B. Provine, _The Evolutionary Synthesis: Perspectives on the Unification of Biology_ (New York: Columbia University Press, 1980), especially pp. 154-157. Gould, though, makes it quite clear that Osborn favored theories derived _exclusively_ from the data of paleontology, and thus was far from endorsing any unified conception of the evolutionary process, especially one which relied heavily on experimental genetics.

223. J.M. Gillette, "Ancestorless Man: The Anthropological Dilemma."

224. Osborn, _The Earth Speaks to Bryan,_ p. 55.

225. Osborn, _Man Rises To Parnassus,_ p. viii.

Chapter II

1. Robert W. Ehrich, "George Grant MacCurdy,1863-1947," American Antiquity 14 (1948): 49-50, p. 49; Hugh Hencken, "Obituary: George Grant MacCurdy, 1863-1947," Science 107 (1948): 639-640, p. 639.

2. Theodore D. McCown, "George Grant MacCurdy, 1863-1947," American Anthropologist 50 (1948): 516-524, p. 516; Hencken, "George Grant MacCurdy, 1863-1947," Bulletin of the American School of Prehistoric Research, no. 16 (1948): v-xxii, p. vi. Both of these biographies contain extensive bibliographies of MacCurdy's writings, especially Hencken's.

3. McCown, "George Grant MacCurdy," pp. 516-517; Hencken, "MacCurdy," Science , p. 639.

4. McCown, "George Grant MacCurdy," p. 517.

5. Ibid., pp. 518-519.

6. Earnest A. Hooton, "George Grant MacCurdy, 1863-1947," American Anthropologist 52 (1950): 513-515, pp. 513-514.

7. Hencken, "MacCurdy," Bulletin of the American School, p. viii.

8. Glyn Daniel, The Idea of Prehistory (Cleveland and New York: World Publishing Co., 1963), pp. 97-101.

9. George Grant MacCurdy, "The Eolithic Problem -- Evidences of a Rude Industry Antedating the Paleolithic," American Anthropologist, n.s. 7 (1905), 425-479, p. 452.

10. MacCurdy, "On the Relation of Archeology to Ethnology from the Quaternary Standpoint," American Anthropologist, n.s. 15 (1913), 567-573, p. 570.

11. MacCurdy, "The Eolithic Problem," p. 468.

12. As we have seen, Osborn took this very course in his campaign for the "Pro-Dawn man" (see the chapter on Osborn pp. 85-86). The major factor in this redating was the Selenka Expedition of 1907-1908, which pushed forward the date of the so-called "Trinil" fauna associated with Java man to the Lower Pleistocene. MacCurdy's longest

discussion of Java man, in Human Origins, took account of this revision, but he showed his lingering reservations by noting that the Trinil fauna still bore a "striking resemblance to that of the Siwalik hills of India, which have been refrerred to the late Pliocene." Indeed, by waffling on the morphological evaluation of "Pithecanthropus" along with the geological, MacCurdy succeeded in avoiding a conclusive placement of the fossil on either side of the Eolithic-Paleolithic line. In his conclusion regarding Java man, he described the three views of that fosil's phylogenetic position -- as a transitional form between pongids and hominids, as a possible representative of the genus Homo which might be ancestral to later forms, and as a true hominid but one which represented only a "side-branch" on the human family tree -- without indicating the alternative that he favored. See Human Origins: A Manual of Prehistory (New York: Appleton, 1924), v.1, pp. 318-319. In the concluding chapter to the first volume of Human Origins he vaguely summarized the position of "Pithecanthropus erectus" as one of "very close" kinship with Homo, and as a creature "which lived during an early phase of the Old Stone Age, and which itself might have been a tool user" (p. 435). In this connection, it is interesting to note that, despite the clear asociation of Homo erectus remains with well-known Paleolithic toll traditions in East Africa and China, conclusive evidence of tool use by the Java form of Homo erectus is still apparently lacking, according to Gert-Jan Bartstra, "Homo erectus erectus : the Search for His Artifacts," Current Anthropology 23 (1982): 169-189. Fuzziness along the Eolithic-Paleolihic boundary was also encouraged by the fact that the famous Mauer jaw, the only other authentic hominid fossil antedating the Neanderthals discovered prior to 1920, had also turned up without accompanying artifacts. Thus, in one of his articles, MacCurdy could subvert his own scheme for correlating cultural and morphological evolution -- by classing "Homo heidelbergensis" as a likely representative of "Eolithic" man, even though he clearly saw the jaw as belonging to the genus Homo . See MacCurdy, "Eolithic and Paleolithic Man," American Anthropologist n.s. 11 (1909): 92-100.

13. See pp. 46-47 and note 25 in the chapter on Osborn. The relevant works by Penck are Albrecht Penck,"Das Alter des Menschengeschlechts," Zeitschrift für Ethnologie 40 (1908): 390-407, and (with E. Bruckner), Die Alpen in Eiszeitalter (Leipzig, 1909).

14. MacCurdy, "Recent Discoveries Bearing on the

Antiquity of Man in Europe," Annual Report of the Smithsonian Institution for 1909 (1910): 531-583, p. 572.

15. MacCurdy, "Penck on the Antiquity of Man," Records of the Past 8 (1909): 33-38, p. 38.

16. Ibid. As noted above (n. 5) MacCurdy did not always make a clear association between the presence of the genus Homo and that of Paleolithic culture -- the "lower Quaternary" date commonly attributed to the Mauer jaw apparently clashed too strongly with the "middle Quaternary" date accepted for the earliest "Chellean" artifacts. This conflict caused MacCurdy to conjecture in 1910 that "Eolithic" artifacts might eventually be found in association with Heidelberg man. MacCurdy, "Recent Discoveries," p. 572.

17. Ibid., p. 575; MacCurdy, "Somatology and Man's Antiquity," Records of the Past 10 (1911): 322-331, p. 327. See note 45 of the Osborn chapter for references to Boule, and also Hammond, "The Expulsion of the Neanderthals."

18. MacCurdy, Human Origins, v. 2, pp. 103, 155.

19. Ibid., p. 103.

20. Ibid., pp. 104, 116.

21. Ibid., pp. 103-104, 155.

22. Both types were unearthed by MacCurdy himself in an excavation of a Mousterian layer at the cave of La Combe in France. See MacCurdy, Human Origins, v. 1, p. 133.

23. MacCurdy, "Eolithic and Paleolithic Man," p. 97.

24. MacCurdy, "Recent Discoveries," p. 583, and also pp. 576-577; see also MacCurdy, "On the Relation of Archeology to Ethnology."

25. MacCurdy, Human Origins, v. 1, pp. 321-322, 332-333, 314-315. The first well-dated fossil found in an Acheulian context was Swanscombe man, about which more will be said in the chapter on Hooton. See p. 396-397 below.

26. The latter fossil was also found in a

Mousterian context, but what was then known as "warm Mousterian," to which MacCurdy, following the consensus of the time, assigned an earlier, Third Interglacial date. MacCurdy, "Interglacial Man from Ehringsdorf near Weimar," American Anthropologist, n.s. 17 (1915): 139-142. Later work cast some doubt upon the "warm Mousterian" designation, but the third interglacial date and thej close similarity of the jaw to the Neanderthal "type" continued to be maintained. MacCurdy, Human Origins, v. 1, pp. 346-349.

27. MacCurdy, Human Origins, v.1, p. 350.

28. Ibid., p. 313.

29. Ibid., p. 379.

30. Ibid.

31. Ibid., p. 141.

32. Ibid., p. 156; a similar contrast is portrayed on p. 378 also.

33. Ibid., pp. 385, 397.

34. Ibid., p. 385. In evolutionary discussion since the impact of the "modern synthesis" reproductive isolation, which by definition imposes a low likelihood of "racial mixture," has been taken as a key attribute of a specific difference between two creatures. This notion of MacCurdy's about the Chancelade fossil is one of the many examples illustrating that the species concept was not understood in the "modern" way by MacCurdy and many of his contemporaries.

35. Ibid., p. 138.

36. Ibid., pp. 161-162.

37. Ibid., v. 2, p. 169.

38. Ibid., v. 1, p. 139.

39. Ibid., p. 325; see n. 69 in the chapter on Osborn for refernces to Symington.

40. Ibid., pp. 325, 378.

41. Ibid., p. 378.

42. Ibid.

43. Ibid.

44. MacCurdy, "Recent Discoveries," p. 572; "Somatology and Nan's Antquity," pp. 330-331.

45. MacCurdy, "Recent Discoveries," p. 572.

46. MacCurdy, "Pleistocene Man from Ipswich (England)," Science n.s. 35 (1912): 505-507; also MacCurdy, "Review of Arthur Keith, The Antiquity of Man," American Anthropologist, n.s. 18 (1916): 111-112, p. 112. These were the Ipswich skeleton, discovered by James Reid Moir of "Foxhallian" culture fame, and the Galley Hill skeleton, unearthed in England during the late 19th century. Both were later established as intrusive burials not contemporaneous with the geological deposits in which they were found. For the Galley Hill issue see Kenneth P. Oakley, "The Problem of Man's Antiquity," pp. 150-151. Marcellin Boule and Henri Vallois, Fossil Men (New York: the Dryden Press, 1957), p. 153 has a concise account of the brief career of Ipswich man.

47. MacCurdy, "Ancestor Hunting: the Significance of the Piltdown Skull," American Anthropologist, n.s. 15 (1913): 248-256, p. 252.

48. Ibid., p. 255.

49. Ibid., p. 256.

50. Ibid., p. 251. MacCurdy was quoting from Elliot Smith, "Presidential Address to Section H (The Evolution of Man)." The ease with which many professionals accepted the odd association of ape-like jaw with human braincase testifies to the forger's understanding of the theoretical expectations to "play to" in constructing the hoax. See Hammond, "A Framework of Plausibility," as well as Stephen Jay Gould, "Piltdown Revisited," Natural History 88 no. 3 (1979): 86-97 for discussions of this point.

51. MacCurdy, "The Man of Piltdown," American Anthropologist n.s. 16 (1914): 331-336, p. 336.

52. Gerrit S. Miller, "The Jaw of Piltdown Man," Smithsonian Miscellaneous Collections 65, no. 12 (1915): 1-31. Miller replied to his critics and defended his

conclusions in "The Piltdown Jaw," American Journal of Physical Anthropology 1 (1918): 25-52.

53. MacCurdy, "The Revision of Eoanthropus Dawsoni," Science n.s. 43 (1916): 228-231, p. 229.

54. Ibid., p. 230.

55. MacCurdy, "Recent Developments in Prehistory," Scientific Monthly 18 (1924): 467-474, p. 467; Human Origins, v. 1, p. 338.

56. MacCurdy, Human Origins, v. 1, p. 338.

57. Ibid.

58. Ibid., p. 340.

59. Ibid.

60. Ibid., p. 329.

61. Ibid., p. 435.

62. Ibid., pp. 318-319.

63. Ibid., p. 431.

64. Ibid., pp. 296-298.

65. Ibid., p. 3.

66. Ibid., p. 431.

67. MacCurdy, The Coming of Man (New York: the University Society, 1932), p. 17.

68. MacCurdy, Human Origins, v. 1, p. 3.

69. MacCurdy, The Coming of Man, p. 7. See also Human Origins, v. 1, p. 431 for similar language implying the primacy of the brain in hominid evolution.

70. Elliot Smith, "Presidential Address to Section H," especially pp. 591, 594-595.

71. See the chapter on Hooton pp. 355-362.

72. See the chapter on Osborn pp. 114-122.

73. To convince oneself that MacCurdy's command over detail was formidable, one need only look at the various lists and catalogues in Human Origins, for example the catalogue summarizing the stratigraphy of every Paleolithic site in Europe, v. 2, pp. 301-349.

74. MacCurdy, "Recent Discoveries," p. 540.

75. MacCurdy, Human Origins, v. 1, p. 405; se also MacCurdy, "On the Relation of Archeology to Ethnology," p. 570.

76. C.D. Matthew, "Review of George Grant MacCurdy, Human Origins, " American Anthropologist, n.s. 27 (1925), 464-467, p. 465; MacCurdy, "Concerning Human Origins," American Anthropologist n.s. 28 (1926), pp. 308-310.

77. Oakley, "The Problem of Man's Antiquity," pp. 125-127, and Hammond, "Anthropology as a Weapon of Social Combat."

78. He followed this procedure not only in Human Origins, v. 1 but also in all of his other synthetic discussions of prehistory -- see MacCurdy, The Coming of Man and "Recent Discoveries," as well as "New Light on the Progress of Primitive Man," Current History 23 (1926): 663-674, and "Recent Progress in the Field of Old World Prehistory," Annual Report of the Smithsonian Institution for 1930 (1931): 495-509.

79. MacCurdy, "The Caveman as Artist," Century, 84 (1912):439-446, p. 441.

80. MacCurdy, "On the Relation of Archeology to Ethnology," p. 572.

81. Kuhn, The Structure of Scientific Revolutions, pp. 23-42.

82. See, for example, MacCurdy, "Recent Discoveries," and "Penck on the Antiquity of Man."

83. MacCurdy, "Penck on the Antiquity of Man," p. 34. Also see MacCurdy's note fo a similar effort with regard to the earlier Paleolithic by the French prehistorian Commont in The Coming of Man, p. 45.

84. See note 33 of the chapter on Osborn.

85. MacCurdy, "Recent Discoveries," pp. 542-543.

86. MacCurdy, <u>The Coming of Man,</u> pp. 495-496.

87. MacCurdy, "The Eolithic Problem," pp. 430-431.

88. MacCurdy, "La Combe, a Paleolithic Cave in the Dordogne," <u>American Anthropologist</u> n.s. 16 (1914): 157-184, p. 184.

89. MacCurdy, "The First Season's Work of the American School in France for Prehistoric Studies," <u>American Anthropologist</u> n.s. 24 (1922): 61-71, p. 61.

90. The effort involved in producing them could even impress those who were critical of the assumptions on which they were based. See Glyn Daniel, <u>A Hundred Years of Archeology</u> (London:Duckworth, 1950), pp. 244-245.

91. MacCurdy, <u>The Coming of Man,</u> p. 48; indeed, Breuil had made his desertion from the old orthodoxy quite explicit by 1931. See Daniel, <u>A Hundred Years,</u> p. 240, and more recently, Lewis R. Binford, <u>In Pursuit of the Past: Decoding the Archeological Record</u> (London: Thames and Hudson, 1983), p. 86. Breuil summarized his new general conclusions in Henri Breuil, "Les Industries à Éclats du Paléolithique Ancien," <u>Préhistoire</u> 1 (1932): 125-190, and "La Paleolithique Ancien en Europe Occidentale et sa Chronologie," <u>Bulletin de la Societe Prehistorique Francaise</u> 29 (1932): 570-578.

92. MacCurdy, <u>The Coming of Man,</u> p. 48.

93. Breuil apparently leaned toward this conclusion himself. See the comments in Francois Bordes, <u>The Old Stone Age</u> (New York: McGraw Hill, 1966), pp. 92, 95, and Binford, <u>In Pursuit of the Past,</u> p. 86.

94. See pp. 145-149 above on this theme in <u>Human Origins,</u> and also <u>The Coming of Man,</u> pp. 91-144.

95. MacCurdy, "On the Relation of Archeology to Ethnology," p. 570; <u>Human Origins,</u> v. 2, p. 139.

96. MacCurdy, "On the Relation of Archeology to Ethnology," p. 578.

97. MacCurdy, "The Eolithic Problem," pp. 468-469.

98. MacCurdy, "Ancestor Hunting," p. 252.

99. MacCurdy, Human Origins, v. 2, p. 139.

100. Ibid., pp. 103, 105.

101. MacCurdy, "New Light on the Progress of Primitive Man," p. 665.

102. Ibid., p. 664.

103. The preeminient work in this vein was of course Franz Boas' classic The Mind of Primitive Man (New York: Macmillan, 1911), but Alfred Kroeber's famous essay "The Superorganic," American Anthropologist, n.s. 19 (1917): 163-213, was a landmark as well.

104. See pp. 144-145 above.

105. MacCurdy, Human Origins , v. 1, pp. 315, 325.

106. See the chapter on Osborn, pp. 41-43.

107. The impact of these two fields of study was not feally felt in American paleoanthroppology until the period 1945-1950, as is clearly demonstrated in three recent analyses of this aspect of the history of physical anthropology -- Erik Trinkaus, "A History of Homo Erectus and Homo Sapiens Paleontology in America," Ernst Mayr,"Reflections on Human Paleontology," and Noel T. Boaz, "American Research on Australopithecus and Early Homo ," all in Frank Spencer, ed., A History of American Physical Anthropology (New York: Academic Press, 1982). The tension between biological and social science is a major theme in Hamilton Cravens, The Triumph of Evolution: American Scientists and the Heredity-Environent Controversy (Philadelphia: University of Pennsylvania Press, 1978).

108. The term "chimaera" was that of Franz Weidenreich. See his powerful attack on the antiquity of Piltdown man in Weidenreich, "The Skull of Sinanthropus: A Comparative Study on a Primitive Hominid Skull," Paleontologia Sinica , new series D, no. 10, whole series, no. 127 (1943), pp. 216-220. Recent discussions of the impact of Piltdown on the early- sapiens theory and other matters of interpretation in human phylogeny include Hammond, "A Framework of Plausibility," and Spencer, "The Neanderthals and Their Evolutionary Significance."

109. In The Coming of Man MacCurdy hazarded the guess that this point of separation between the two lines

might reach as far back as the Pliocene. See table on page 21. A vigorous, and controversial, account of the problems caused by Boule's monograph is C.L. Brace, "The Fate of the 'Classic' Neanderthals: A Consideration of Hominid Catastrophism." Recently, Erik Trinkaus has argued that Boule's interpretation of the Neanderthal post-cranial skeleton was not so strained, and was more in line with then undertstood canons of vertebrate paleontology, than Brace and other critics have alleged. See Trinkaus, "A History of Homo Erectus and Homo Sapiens Paleontology," p. 264. Michael Hammond makes a similar case even more convincingly, see Hammond, "The Expulsion of the Neanderthals," pp. 13-16.

110. For an interesting discussion of the initial reception of Australopithecus, and the eventual shift in interpretationof the australopthecines by a key participant in the latter process, see. W.E. LeGros Clark, Man-Apes or Ape-Men (New York: Holt, Rinehart and Winston, 1967). An evaluation of the situation in the U.S. is provided by Boaz, "American Research on Austraolpithecus."

111. MacCurdy, The Coming of Man, pp. 16-17.

112. MacCurdy, "Recent Progress in the Field," pp. 500-501.

113. Ibid.

114. See, for example, standard works like Bordes, The Old Stone Age, pp. 84-86, and R.G. Braidwood, Prehistoric Men (Chicago: University of Chicago Press, 1967), pp. 35-47. According to MacCurdy, Breuil went so far as to claim that the Sinanthropus culture showed that creature to be "not inferior in mentality to the Neanderthalians." MacCurdy, "New Light on Prehistoric Man in Asia," Proceedings of the American Philosophical Society 74 (1934): 185-191, p. 186.

115. MacCurdy, "New Light in Asia," p. 186.

116. MacCurdy, "Prehistoric Man in Palestine," Proceedings of the American Philosophical Society 76 (1936): 524-541, pp. 525-526.

117. The full description of these fossils is in Theodore McCown and Sir Arthur Keith, The Stone Age of Mt. Carmel. Volume 2: the Fossil Human Remains from the Levalloiso-Mousterian, (London: Oxford University Press, 1939).

118. MacCurdy, "Prehistoric Man in Palestine," p. 524.

119. Ibid., p. 527.

120. Ibid., p. 537.

121. Ibid., pp. 541, 536.

122. Ibid., p. 536.

123. MacCurdy, "Prehistoric Research in the Near East," Proceedings of the American Philosophical Society 72 (1933): 121-135, p. 133.

124. MacCurdy, "Prehistoric Man in Palestine," pp. 530-531.

125. MacCurdy, "Prehistoric Man in Palestine," pp. 531-532. For a recent analysis see Erik Trinkaus, "Western Asia," in Smith and Spencer, The Origins of Modern Humans.

126. MacCurdy, "Prehistoric Man in Palestine," p. 536. Keith and McCown said the following about the variability of the Skhūl population: "if these individuals had been found in different sites at different times, and each one described by a different author, we should have had a corresponding number of fossil races." McCown and Keith, "Mt. Carmel Man and his Bearing on the Ancestry of Modern Races," Bulletin of the American School of Prehistoric Research, no. 13 (1937): 5-16, p. 14.

127. MacCurdy,"Prehistoric Man in Palestine," p. 536.

128. Ibid.

129. Ibid.

130. Ibid., p. 525.

131. Kuhn, The Structure of Scientific Revolutions, pp. 35-42.

132. MacCurdy, "Recent Discoveries," pp. 548-560, and MacCurdy, "Old World Prehistory in Retrospect and Prospect," Proceedings of the American Philosophical Society 68 (1929): 95-106, are both good examples of the

importance he gave to art finds in his synthetic accounts.

133. MacCurdy, "The Caveman as Artist," pp. 439-441, and passim ; "The Field of Paleolithic Art," American Anthropologist, n.s. 26 (1924): 27-50; and "The Dawn of Art: Cave Paintings, Engravings and Sculptures," Art and Archaeology 4 (1916): 71-90.

134. About 95 pages worth of text.

135. MacCurdy, "The First Season's Work of the American School," p. 69.

136. MacCurdy, "On the Relation of Archeology to Ethnology," p. 571.

137. Ibid.

138. Ibid., pp. 571-572; MacCurdy, "The Dawn of Art," pp. 77-83.

139. P. Ucko and A.Rosenfeld provide an interesting survey of Paleolithic art that also presents critical analysis of earlier attempts to explain the phenomenon. Though MacCurdy is not mentioned specifically by them, on this issues and other he was expressing views common among archeologists in his time. See Peter Ucko and Andrea Rosenfeld, Paleolithic Cave Art (London: Weidenfield and Nicholson, 1967).

140. MacCurdy, Human Origins, v. 2, pp. 248-249.

141. Ucko and Rosenfeld, Paleolithic Cave Art, pp. 116-122.

142. See pp. 58-59 above.

143. MacCurdy, "The Dawn of Art," p. 88.

144. MacCurdy, "The Caveman as Artist," pp. 439-440.

145. Ibid., 439; also see MacCurdy, "Recent Discoveries," pp. 566-568.

146. George W. Stocking, Jr., Race, Culture and Evolution: Essays in the History of Anthropology (New York: Free Press, 1968).

147. Ucko and Rosenfeld, Paleolithic Cave Art , pp.

123-124.

148. He did note, however, that one modern group of "primitives", the Bushmen of South Africa, did practice a similar type of art, perhaps because they were a remnant population which had descended from the Upper Paleolithic peoples of North Africa and had been able to perpetuate the artistic tradition by migrating to the isolated desert environment of South Africa. MacCurdy, "The Caveman as Artist," p. 448.

149. Ucko and Rosenfeld, Paleolithic Cave Art, pp. 123-124.

150. Ibid., pp. 128-130.

151. MacCurdy, "Recent Discoveries," p. 562. At the cave of Niaux there were mural representations of bisons with "arrows sticking in their sides. It is suggested that these may be votive figures symbolizing the hunter's hopes for success in the chase." See also MacCurdy, "Paleolithic Art as Represented in the Collections of the American Museum of Natural History," American Museum Journal 14 (1914): 225-237, and "The Caveman as Artist," p. 444.

152. MacCurdy, "The Field of Paleolithic Art," and Human Origins, v. 1, pp. 253-254.

153. MacCurdy, "The Field of Paleolithic Art," p. 45.

154. Ibid.

155. MacCurdy, Human Origins, v. 1, pp. 288-291.

156. Ibid.

157. MacCurdy, "On the Relation of Archeology to Ethnology," p. 570; see also Human Origins, v. 1, pp. 231-232, and The Coming of Man, p. 93.

158. Ucko and Rosenfeld, Paleolithic Cave Art , pp. 174-195 contains extensive critical analysis of this evidence. The foremost recent French student of Paleolithic cave art has also concluded that the "sympathetic magic" interpretation is based on an incomplete analysis of the iconography of the cave murals. See André Leroi-Gourhan, The Dawn of European Art: An Introduction to Paleolithic Cave Painting, translated by Sara Champion (Cambridge: Cambridge Unversity Press,

1982), pp. 43-76.

159. In fact, the idea that the efficiency of later Paleolithic hunting and gathering and population growth that resulted from it were major factors in a "food crisis" which ushered in the transition to Neolothic agriculture is stil argued today, though the hypothesis is controversial. See for example Mark N. Cohen, The Food Crisis in Prehistory: Overpopulation and the Origins of Agriculture (New Haven: Yale University Press, 1977).

160. See pp. 117-118 above.

161. Interestingly, Ucko and Rosenfeld note that in Breuil's work the magical theory did to occupy center stage, but was rather a sidelight to his major efforts concerning Upper Paleolithic art -- tracing the evolution of styles and motifs, and dating them with as much precision as possible. Ucko and Rosenfeld, Paleolithic Cave Art, p. 130.

162. MacCurdy, Human Origins, pp. 232-235.

163. MacCurdy, "Recent Progress in the Field Of Old World Prehistory," Annual Report of the Smithsonian Institution for1930 (1931): 495-509.

164. Compare Human Origins, v. 1, pp. 228-232 on the differences between Upper Paleolithic art and that of modern "primitives" with a similar passage in "The Dawn of Art," p. 78-80; also see the passage on Breuil's account of the evolutionof mural styles with a similar passage in the same article, pp. 75-77.

165. MacCurdy, Human Origins, v. 1, pp. 265-293.

166. MacCurdy, "The Eolithic Problem -- Evidences of a Rude IndustryAntedating the Paleolithic," American Anthropologist, n.s. v. 7 (1905): 425-479.

167. S.J. Gould and Niles Eldredge, "Punctuated Equilibria: the Tempo and Mode of Evolution Reconsidered," Paleobiology 3 (1977): 115-151, is a good place to start in exploring the "anti-gradualist" critique. Ruse, Darwinism Defended, pp. 212-226 discusses the pros and cons of the issues but shades toward the qradualist position.

168. Gradualism, of course, was not the only traditional mode of thought implicit in concepts like

these. Associationist psychology also left its mark, a fact which helps explain the ease with which MacCurdy could invoke a simple associationist learning model explain cultural advances which must, at least in part, have required biological changes like increased brain size.

169. J.C. Merriam, "Review of 'The Eolithic Problem,' by G. Grant MacCurdy," Science, n.s. 23 (1906): 659-661.

170. An extensive description and analysis of the "Oldowan" artifats at the "type site" of Olduvai Gorge in Tanzania is contained in M.D. Leakey, Olduvai Gorge, v. 3 (Cambridge: Cambridge University Press, 1971).

171. See p. 229 below.

172. Oakley, "The Problem of Man's Antiquity," p. 102.

173. MacCurdy, "The Eolithic Problem," p. 470.

174. Henry Fairfield Osborn, Man Rises to Parnassus, pp. 48-76.

175. MacCurdy, "The Eolithic Problem," p. 433.

176. Ibid., p. 438.

177. Ibid., p. 464.

178. Oakley, "The Problem of Man's Antiquity," p. 141.

179. MacCurdy, "The Eolithic Problem," p. 464.

180. Oakley, The Problem of Man's Antiquity, p. 133, n. 27.

181. See p. 141 above.

182. MacCurdy, "The Eolithic Problem," p. 428.

183. Merriam, "Review of 'The Eolithic Problem,'" p. 660.

184. As late as 1919, Merriam himself was suspending judgment on this question, saying at once that it was "not improbable" for creatures like Java and

Heidelberg man to have used "eoliths," but also admitting that many of these stones, especially in the lower "eolith beds" might have been formed "without human assistance." J.C. Merriam, "The Beginnings of Human History Read from the Geological Record: the Emergence of Man," Scientific Monthly 9 (1919): 193-209, 10 (1920): 321-342, 425-437; see v. 10, pp. 340-341.

185. MacCurdy, "The Eolithic Problem," p. 449.

186. Ibid.

187. Ibid., p. 452.

188. Ibid., p. 454.

189. See for example MacCurdy, "Eolithic and Paleolithic Man," and "Ancestor Hunting."

190. MacCurdy, Human Origins, v. 1, p. 102.

191. Ibid., pp. 97-100.

192. Henri Obermaier, Fossil Man in Spain (New Haven: Yale University Press, 1924), p. 15. Obermaier's remarks on eoliths in this work are especially interesting since they were published in America in the very year that Human Origins appeared, and show how exactly the same body of evidence could be used to support precisely the opposite set of conclusions from the ones MacCurdy was defending.

193. MacCurdy, Human Origins, v. 1, pp. 91-92.

194. Obermaier, Fossil Man, pp. 15-17.

195. MacCurdy, Human Origins, v. 1, p. 101.

196. Obermaier, Fossil Man, p. 17-18; MacCurdy, Human Origins, v. 1, p. 102.

197. Obermaier, Fossil Man, p. 15.

198. Obermaier, Fossil Man, p. 18; MacCurdy, Human Origins, v. 1, p. 102.

199. MacCurdy, "New Light on the Progress of Primitive Man," passim .

200. MacCurdy, "Old World Prehistory in Retrospect

and Prospect," for example, has no mention of pre-Paleolithic matters.

201. MacCurdy, The Coming of Man, p. 49.

202. Ibid., pp. 91-144; see pp. 179-181 above.

Chapter III

1. Frank Spencer, "Aleš Hrdlička, M.D., 1869 - 1943: a Chronicle of the Life and Work of an American Physical Anthropologist," (Ph. D. Dissertation, University of Michigan, 1979), pp. 3, 16-17, 23-24.

2. Ibid., pp. 29, 35, 42-44.

3. Ibid., pp. 46, 50, 52, 98-99.

4. Ibid., pp. 105-116. It is interesting to note that Manouvrier was much more receptive to environmental rather than hereditarian explanations of deviant psychology and disease than Broca and most of his followers. In addition Manouvrier was a vigorous critic of the various "anthroposociological" theorists who promoted "scientific racism" as a key to understanding the social and political problems of the time (Spencer pp. 116 -119). It is also worth mentioning that Manouvrier also opposed the notion, common among biologists of the time, that the brains of women were biologically inferior to those of men (Gould, The Mismeasure of Man, pp. 106-107). These sceptical, liberal views were probably an important factor in the idealistic Hrdlička's attraction to his mentor.

5. Spencer, "Aleš Hrdlička, M.D.," pp. 133, 163-169, 172-173.

6. Ibid., pp. 206, 234-240; Aleš Hrdlička, "The Crania of Trenton, New Jersey, and their Bearing upon the Antiquity of Man in That Region," Bulletin of the American Museum of Natural History, 16 (1902): 23-62, and "The Lansing Skeleton," American Anthropologist, n.s.5 (1903): 323-330.

7. Spencer, "Aleš Hrdlička, M.D.," pp.241-243, 248-251.

8. Hrdlička, Skeletal Remains Suggesting or Attributed to Early Man in North America. Bulletin of the Bureau of American Ethnology, no. 33 (Washington: Smithsonian Institution, 1907); Hrdlička (in collaboration with W.H. Holmes, B. Willis, F.E. Wright, and C.N. Fenner), Early Man in South America. Bulletin of the Bureau of American Ethnology, no. 102 (Washington: Smithsonian Institution, 1912). See, in particular, Frank Spencer and Fred H. Smith, "The Significance of Aleš Hrdlička's 'Neanderthal Phase of Man': a Historical and Current Assessment," American Journal of Physical Anthropology, 56 (1981):435-458, pp. 439-442.

9. Spencer and Smith have made the same judgement about the morphological dating charge as well -- "'Neanderthal Phase,'" p. 437.

10. Hrdlička, Early Man in North America, pp. 13-14.

11. Hrdlička, Early Man in North America, p. 14.

12. Hrdlička, Early Man in North America, pp. 12-13, as well as Spencer and Smith, "'Neanderthal Phase,'" p. 437.

13. Spencer and Smith, "'Neanderthal Phase,'" p. 437-438. In addition, Hrdlička initiated studies during the period around 1910 on skeletal material from Egypt, studies which convinced him that evolutionary change in the human form had occurred in historical times among a population whose racial makeup had been relatively stable. Such evidence of morphological "instability" could only have confirmed Hrdlička's conviction that Homo sapiens was a recent arrival in geological terms. See Spencer and Smith, p. 437.

14. See pp. 273-276 below.

15. Spencer and Smith, "'Neanderthal Phase,'" p. 439-442.

16. Hrdlička, "The Most Ancient Skeletal Remains of Man," Annual Report of the Smithsonian Institution for 1913 (1914): 491-552, p. 492.

17. Hrdlička, "Most Ancient Skeletal Remains," pp. 493-494.

18. Hrdlička, "Most Ancient Skeletal Remains," p.

494.

19. Ibid.

20. Hrdlička, "Most Ancient Skeletal Remains," p. 499. The chronicle of Hrdlička's attempts to get at DuBois, who held the "Pithecanthropus" remains "incommunicado" for two decades, makes humorous reading. See Spencer, "Aleš Hrdlička, M.D.," Chapter 10.

21. Hrdlička, "Most Ancient Skeletal Remains," p. 500.

22. Ibid., p. 511.

23. See pp. 260-262 below.

24. Hrdlička, "Most Ancient Skeletal Remains," p. 520.

25. Ibid., p. 530 - 531.

26. Ibid., p. 530.

27. Ibid., p. 525.

28. Ibid.

29. Ibid., p. 550.

30. Ibid., pp. 541, 539.

31. Ibid., pp. 543-544.

32. See the chapter on Osborn, pp. 65-66, and that on MacCurdy, p. 152 for their acceptance of the "simian" characters in the Neanderthals.

33. Hrdlička, "Most Ancient Skeletal Remains," p. 509.

34. Hrdlička, "Anatomical Observations on a Collection of Orang Skulls from Western Borneo," Proceedings of the U.S. National Museum, 31 (1907): 539-568, p. 560.

35. Hrdlička, "Human Dentition from the Evolutionary Standpoint," Dominion Dental Journal, 23 (1911):403-422. The quotation is taken from Spencer, "Ales Hrdlička, M.D.," p. 377.

36. Spencer, "Aleš Hrdlička, M.D.," pp. 377, 379-380.

37. Spencer, "Aleš Hrdlička, M.D.," pp. 479-480.

38. Spencer, "Aleš Hrdlička, M.D.," pp. 496-497; Hrdlička, "Shovel Shaped Teeth," American Journal of Physical Anthropology, 3 (1920): 429-465.

39. Spencer and Smith, "'Neanderthal Phase,'" p. 440.

40. Ibid.

41. Ibid.; Osborn, of course was concerned not only with the replacement of the western European Neanderthals by migrants from the east, but also with confirming his "central Asian theory" of mammalian distribution in general. See pp. 109-112 above.

42. Hrdlička, "The Peopling of Asia," Proceedings of the American Philosophical Society, 60 (1921): 535-545, p. 537.

43. Ibid., p. 541.

44. Ibid., p. 537.

45. Ibid., p. 539.

46. Ibid., p. 538.

47. Ibid., p. 544.

48. Ibid., p. 545.

49. Hrdlička, "The Peopling of the Earth," Proceedings of the American Philosophical Society, 65 (1926): 150-156, p. 150.

50. Ibid., p. 151.

51. Ibid., p. 151-153.

52. See pp. 289-293 below.

53. See pp. 149-152 in the chapter on MacCurdy for a statement of the replacement theory in its most typical form.

54. F.C. Howell, "The Evolutionary Significance of Variation and Varieties of 'Neanderthal' Man," Quarterly Review of Biology, 32 (1957): 330-347, p. 343.

55. F.C. Howell, "The Place of Neanderthal Man in Human Evolution," American Journal of Physical Anthropology, n.s. 9 (1951): 397 - 416.

56. Hrdlička, "Peopling of the Earth," p. 152.

57. Spencer, "Aleš Hrdlička, M.D.," p. 520-526.

58. See for example pp. 334-340 below.

59. Gerrit S. Miller, "The Piltdown Jaw."

60. See especially W.P. Pycraft,"The Jaw of Piltdown Man: a Reply to Mr. Gerrit S. Miller" Science Progress 11 (1917): 389-409; Grafton Elliot Smith,"The Problem of the Piltdown Jaw: Human or Subhuman," Eugenics Review 9 (1917): 167; and Arthur Smith Woodward, "Fourth Note on the Piltdown Gravel, with Evidence of a Second Skull of Eoanthropus Dawsoni ," Quarterly Journal of the Geological Society of London 73 (1917): 1-10.

61. Stephen J. Gould, "Piltdown Revisited," p. 108-124 in The Panda's Thumb: More Reflections in Natural History (New York: W.W. Norton, 1980).

62. Spencer, "Aleš Hrdlička, M.D.," p. 531. Spencer also notes that the instruments that Hrdlička designed for his measurements of molar teeth have become standard among physical anthropologists. His system of nomenclature for small surface details of tooth form was not so successful, and indeed he was criticized by William K. Gregory, who felt that Hrdlička had unjustly ignored Gregory's own elaboration of the Cope-Osborn system of molar tooth nomenclature. Gregory also felt Hrdlička had been particularly unfair in failing to take note of Gregory's priority in the discovery of molar tooth homologies between humans and great apes -- the so called "Dryopithecus pattern." Ibid., p. 539-542.

63. Ibid., p. 536 - 538.

64. Ibid., p. 544 - 546; Hrdlička, "New Data on the Teeth of Early Man and Certain Fossil European Apes," American Journal of Physical Anthropology 7 (1924):109-132, p. 122.

65. Hrdlička, "The Piltdown Jaw," American Journal of Physical Anthropology 5 (1922): 337 - 347; Hrdlička, "Recent Discoveries of Ancient Man in Europe," Smithsonian Miscellaneous Collections 74 (1923): 82-85.

66. Hrdlička, "The Piltdown Jaw," pp. 337-338.

67. Ibid., p. 339.

68. Ibid., pp. 338-340.

69. Ibid., p. 342.

70. Ibid., pp. 345-347; Spencer, "Aleš Hrdlička, M.D.," pp. 536-538.

71. Hrdlička clearly felt sensitive about giving the implication that Miller's earlier judgement had been ill conceived; thus in his last major account of Piltdown, published in 1930, he excused Miller by noting that the latter's "careful study" had been based on casts alone, while he had had the opportunity to work with the specimens themselves. Hrdlička, "The Skeletal Remains of Early Man," Smithsonian Miscellaneous Collections 83 (1930): 1-379, p. 89.

72. Hrdlička, "Skeletal Remains of Early Man," p. 89.

73. Ibid. p. 90.

74. Ibid.

75. Hrdlička, "The Neanderthal Phase of Man," Journal of the Royal Anthropological Institute of Great Britain 57 (1927): 249-274.

76. Ibid., p. 269.

77. Ibid., pp. 269-270.

78. Ibid., p. 251.

79. See for example, pp. 70-72 in the chapter on Osborn, and pp. 169-171 in that on MacCurdy.

80. Hrdlička, "Neanderthal Phase," pp. 267-268.

81. Ibid., p. 252-253. It is worth noting that

around the same time that Hrdlička gave his lecture, Osborn was trying to work up a charatcteristic faunal context for "Neanderthaloids." See the chapter on Osborn, p. 111.

82. Hrdlička, "Neanderthal Phase," p. 253.

83. This data was apparently drawn from the comprehensive survey of Paleolithic sites included by MacCurdy in Human Origins, v. 2, pp. 301-349.

84. Hrdlicka, "Neanderthal Phase," p. 259.

85. Ibid., p. 260. Like MacCurdy, Hrdlička used "Aurignacian" as a general term for the earliest phase of Upper Paleolithic culture. Since the 1920s the archeological record and its terminology have become more complex; there are several distinct tool traditions now recognized as early Upper Paleolithic in Europe. Still, as Spencer and Smith point out, two of these traditions, the Chatelperronian and the Szeletian, do appear to have developed indigenously out of the Mousterian as Hrdlička argued in 1927. See Spencer and Smith, "'Neanderthal Phase,'" p. 446.

86. Hrdlička, "Neanderthal Phase," p. 261.

87. Ibid., p. 262.

88. Ibid., p. 268.

89. Ibid.

90. Ibid., pp. 268-269. As Keith's evaluation of the Galilee skull indicates, he was even at this relatively early date beginning to shift away from a categorical position on the early Pleistocene appearance of modern Homo sapiens. Full public reversal of his position, however, would not come until the mid 1930s. See for example, Keith, "Pursuing the Origin of Races, Science Pursues a New Trail," New York Times, January 20, 1935, sec. 9, p. 11, and Keith, An Autobiography , pp. 629-630. The description of the Galilee Skull was by its discoverer was F. Turville Petre,"Researches in Prehistoric Galilee, 1925-1926," p. 1-52 in Researches in Prehistoric Galilee (London: British School of Archaeology in Jerusalem, 1927).

91. Hrdlička, "Neanderthal Phase," p. 268. Recent research on the latter character, the so-called "occipital

bun", indicates that Hrdlicka may have been pretty accurate in his judgement. See Erik Trinkaus and Marjorie LeMay, "Occipital Bunning Among Later Pleistocene Hominids," American Journal of Physical Anthropology 57 (1982):pp. 27 - 35, p. 32.

92. Hrdlička, "Neanderthal Phase," p. 269.

93. Hrdlička believed that there had been only one major interglacial period, and that most of the later Pleistocene had been taken up by a single glacial epoch, irregular in intensity, but on the whole becoming increasingly severe until its end. Recent opinion, however, puts the appearance of the Aurignacian at a milder, interstadial period of the last glaciation (Spencer and Smith, "Neanderthal Phase,'" p. 447). The key point to remember here, however, is that Hrdlička's opponents did not have access to this more sophisticated chronology either, so that his criticism of their theories still made sense.

94. Hrdlička, "Neanderthal Phase," pp. 260, 270-271. It is interesting to note that Charles Loring Brace, an admirer of Hrdlička's "Neanderthal Phase" theory, made a similar argument an important part of his own unilinear theory of later hominid evolution during the 1960s. See Brace, The Stages of Human Evolution (Englewood Cliffs, New Jersey: Prentice-Hall, 1967), passim.

95. Hrdlička, "Neanderthal Phase," p. 271.

96. Ibid.

97. Ibid., pp. 267, 272. See Keith and McCown, The Stone Age of Mount Carmel, pp. 13-14.

98. Hrdlička, "Neanderthal Phase," p. 272, and Spencer and Smith,"'Neanderthal Phase,'" p. 437.

99. Hrdlička, "Neanderthal Phase," p. 271.

100. Ibid.

101. Ibid., pp. 271-272.

102. Spencer and Smith ("'Neanderthal Phase'", passim .) point out several ways in which Hrdlička's argument still remains persuasive, but the focus here is different -- i.e. on the reasons why the argument failed

to persuade many people in Hrdlicka's day.

103. Spencer has noted a reluctance on Hrdlicka's part to criticize Boule's evaluation of the Neanderthal skeleton as early as 1914 -- thus Hrdlička dropped a passage in the draft of "Most Ancient Skeletal Remains" critical of Boule's "exaggerations." See Spencer, "Aleš Hrdlička, M.D.," p. 343.

104. See, for example, pp. 360-361 in the chapter on Hooton, and note 97 above.

105. Hrdlička, "Neanderthal Phase," p. 263.

106. Ibid., p. 266.

107. Franz Weidenreich, Der Schädelfund von Weimar-Ehringsdorf (Jena: Fischer, 1928); F. Berckenheimer,"Ein Menschenschädel aus den Diluvialen Schottern von Steinheim an der Murr," Anthropologischer Anzeiger 10 (1933): 318-321; and McCown and Keith, The Stone Age of Mt. Carmel .

108. As the surrounding chapters make clear, the four glaciation, three interglacial model was accepted after about 1915 without much criticism by Hooton, MacCurdy, Osborn and Gregory.

109. And of course in 1935 a new fossil would be discovered in England, at Swanscombe, that would keep the hopes of early sapiens theorists alive for a good deal longer. See for example Hooton's use of the fossil, p. 392 in the chapter on Hooton. That Hrdlicka had failed to convince early sapiens theorists that he had provided any "new" evidence on the issue is suggested by Elliot Smith's review of the former's address. See Elliot Smith, "Neanderthal Man as a Distinct Species," Nature 121 (1928): 141.

110. Hrdlička, "Skeletal Remains of Early Man," pp. 328-349.

111. Ibid., especially pp. 344-348.

112. Ibid., pp. 86-90.

113. See p. 248 above.

114. Hrdlička, "Skeletal Remains of Early Man," p. 56.

115. Ibid.

116. Ibid.

117. See the chapter on Osborn, p. 111 for an example of this use of Rhodesian Man as an African "Neanderthaloid".

118. Hrdlička, "Skeletal Remains of Early Man," p. 123.

119. Ibid., p. 129.

120. Ibid., p. 116. It is important to understand the amount of care that had gone into these judgements. Hrdlička had studied the original specimens of Rhodesian Man at the British Museum; on his visit to Africa in 1925 -- a visit that also took him on a brief fossil hunting expedition to the Taungs site and afforded him the oportunity to examine the first australopithecine fossil -- he was also able make observations at the site of the Broken Hill discoveries.

121. Hrdlička, "Skeletal Remains of Early Man," p. 130.

122. Ibid., p. 367. Davidson Black, "Asia and the Dispersal of the Primates."

123. Hrdlička, "Skeletal Remains of Early Man," p. 368.

124. Ibid.

125. Spencer, "Ales Hrdlička, M.D.," p. 598.

126. Hrdlička, "Important Paleolithic Find in Central Asia," <u>Science</u> 90 (1939):296 - 298, pp. 296-297.

127. Hrdlička, "Organic Evolution: Its Problems and Perplexities," <u>Science</u> 71 (1930): 230-233, p. 231.

128. Ibid., p. 232.

129. Hrdlička, "Organic Evolution," p. 232; on the influence of DeVriesian ideas, see Peter J. Bowler, <u>The Eclipse of Darwinism.</u>

130. Hrdlička, "Some Reflections Regarding Human

Heredity," _Proceedings of the American Philosophical Society_ 75 (1935): 295-312, p. 300.

131. Ibid., pp. 300-302; Hrdlička, "Human Typogeny," _Proceedings of the American Philosophical Society_ 78 (1938):79-95, p. 86.

132. Hrdlička, "On the Relations of Anthropology and Psychology," _Science_ 51 (1919): 199-201, p. 201. A similar statement of goals is also expressed in Hrdlička's inaugural article in the _American Journal of Physical Anthropology,_ "Physical Anthropology: Its Scope and Aims,"1 (1918): 3-23, p. 4.

133. See Spencer, "Aleš Hrdlička, M.D.," p. 531 ff.

134. Hrdlička, "Physical Anthropology: Scope and Aims," p. 20.

135. See pp. 257-258 above; in addition, Hrdlička had definite views about the future biological, and social, evolution of humanity that were based upon extrapolation of these trends. See Hrdlička, "Man's Future in the Light of His Past and Present," _Proceedings of the American Philosophical Society_ 68 (1929): 1-11.

136. See Hrdlička, "Normal Variation," _Proceedings of the American Philosophical Society_ 74 (1934): 253-261, p. 258 for an expression of this attitude toward statistics. A similar instance of methodological conservatism can be found in Hrdlička's study of primate brain size. On the one hand he exercised great care in analyzing the various factors that influenced the reliability of brain weight measurements. However, when it came to the important issue of the relationship between brain weight and body size, Hrdlička confined himself to simple ratios, and failed to investigate more complex mathematical relationships, such as the exponential functions that had been in use among European scientists since the 1890s. He thus could only make the following conclusion: "what is plain is that both the absolute and relative size of the brain differs considerably among the primates, and thus far there appears little regularity or law in these differences." Hrdlička, "Weight of the Brain and of the Internal Organs in American Monkeys; wih Data on Brain Weight in Other Apes," _American Journal of Physical Anthropology,_ v. 8 (1925): 201-211, p. 207.

137. Hrdlička, "The Full-Blood American Negro," _American Journal of Physical Anthropology_ 12 (1929): 15 -

33; Hrdlička, "Human Typogeny."

138. Hrdlička, "Human Races," pp. 156-183 in E.V. Cowdry ed., Human Biology and Racial Welfare (New York: Paul Hoeber, 1930), pp. 159, 169. See the chapter on Hooton, pp. 378-379.

139. Hrdlička, "Anthropological Studies in Southern Asia, Java, Australia, and South Africa," Smithsonian Miscellaneous Collections 78, no. 1 (1927): 58-80, p. 75. See the chapter on Hooton, pp. 325-326.

140. Hrdlička, "Human Races," p. 148.

141. On Hrdlička's Czech background and his pride in it, see Spencer, "Aleš Hrdlička, M.D.," p. 3-29.

142. Hrdlička, "Human Races," pp. 179 - 181; Osborn ,of course, put an even greater stress on this concept. See p. 112 above.

143. Hrdlička, "Human Races," p. 180.

144. Hrdlička, "Man's Future," p. 7.

145. Hrdlička, "Children Running on All Fours," American Journal of Physical Anthropology 11 (1928): 149-185, p. 178.

146. Hrdlička, "Human Heredity," pp. 303 - 304; Hrdlička, "The Problem of Human Evolution," pp. 17-43 in Ruth Anshen ed., Science and Man (New York: Harcourt, Brace, 1942).

147. Hrdlička, "The Forehead," Proceedings of the American Philosophical Society 72 (1933): 315 - 332, p. 318.

148. Hrdlička, "Measurements of 100 Members of the Academy and What They Show," Science 69 (1929): 503.

149. On the interest of Broca et al. in the craniometery of eminient men, see Gould, The Mismeasure of Man . p. 88-94.

150. Hrdlička, "The Problem of Human Evolution," p. 36; neo-Lamarckian language is also contained in Hrdlička, "The Evidence Bearing on Man's Evolution," Smithsonian Annual Report for 1927 (1928): 417-432, pp. 426-427, and in "Organic Evolution: It's Problems and Perplexities."

151. Hrdlička, "Evidence Bearing on Man's Evolution," p. 426. Osborn also made use of the "Baldwin effect," or principle of organic selection, and in fact claimed joint priority with Baldwin in its discovery. See Bowler, The Eclipse of Darwinism, pp. 131-132.

152. Hrdlička, "Evidence Bearing on Man's Evolution," p. 426; "Problem of Human Evolution," p. 36.

153. Hrdlička, "Human Heredity," p. 307.

154. Hrdlička, "Human Heredity," p. 310-311; "Man's Future," pp. 7-9.

155. Spencer, "The Rise of Academic Physical Anthropology in the United States: a Historical Overview," American Journal of Physical Anthropology 56 (1981): 353-364.

Chapter IV

1. William W. Howells, "Memorium: Earnest Albert Hooton," American Journal of Physical Anthropology 12 (1954): 445-453, pp. 445-446.

2. Frank Spencer, "The Rise of Academic Physical Anthropology in the United States (1880-1980): A Historical Overview," American Journal of Physical Anthropology 56 (1981): 353-364, p. 361.

3. Ibid.

4. Harry L. Shapiro, "Earnest Albert Hooton, 1887-1954: In Memoriam Cum Amore ," American Journal of Physical Anthropology 56 (1981): 431-434, p. 433.

5. Ibid.

6. Ibid., p. 434.

7. Edward E. Hunt, Jr., "The Old Physical Anthropology," American Journal of Physical Anthropology 56 (1981): 339-346, p. 344. The relevant works by Hooton are The Indians of Pecos Pueblo: a Study of Their Skeletal Remains (New Haven: Yale University Press, 1930); The

Ancient Inhabitants of the Canary Islands," Harvard African Studies, volume 7 (Cambridge: Harvard University Press, 1925); The American Criminal (Cambridge: Harvard University Press, 1939); and Hooton, with C.W. Dupertuis and H. Dawson The Physical Anthropology of Ireland (Cambridge: Harvard University Press, 1955).

8. Ibid.

9. George J. Armelagos, David S. Carlson, and Dennis P. Van Gerven, "The Theoretical Foundations and Development of Skeletal Biology," pp. 306-316 in Frank Spencer, ed., A History of American Physical Anthropology, 1930-1980 (New York: Academic Press, 1982); Hunt, "The Old Physical Anthropology," p. 344.

10. Armelagos et al., "Theoretical Foundations," p. 309.

11. Hunt, "The Old Physical Anthropology," p. 345; Armelagos et al., pp. 317-319.

12. See e.g. Hooton, "Note on the La Quina Skull," American Anthropologist, n.s. 16 (1914): 267-268, and "The Evolution of the Human Face and its Relation to Head Form," Dental Cosmos 13 (March, 1916): 12.

13. I refer here to the fact that the period 1924-1927 saw the publication of MacCurdy's Human Origins, Osborn's first full statement of his "Pro-Dawn Man" theory, and Hrdlicka's classic exposition of his "Neanderthal Phase of Man" theory.

14. Earnest A. Hooton, Up From the Ape (New York: The Macmillan Company, 1931).

15. Hooton, "The Asymmetrical Character of Human Evolution," American Journal of Physical Anthropology 8 (1925): 125-141.

16. See for example pp. 277-278 above.

17. Arthur Keith, "Modern Problems Relating to the Antiquity of Man," Lancet 183 (1912): 807-810, p. 810.

18. Keith, The Antiquity of Man (London: Williams and Norgate, 1915).

19. Keith, "Darwin's Theory of Man's Descent As It Stands Today," Nature 120, supplement (1927): 14-21, p.

17.

20. Ibid.

21. Hooton, "Asymmetrical Character," p. 137.

22. Ibid., pp. 126-127; the type of distortion I am inferring here has afflicted craniometry from its beginning as a "science". Stephen J. Gould, in his excellent The Mismeasure of Man (New York: Norton, 1981), gives numerous examples.

23. Hooton, "Asymmetrical Character," pp. 138-140.

24. Ibid., p. 138.

25. Ibid.

26. Ibid., pp. 138-139.

27. We have seen MacCurdy hint at similar processes; see pp. 152-153 above.

28. Hooton, "Asymmetrical Character," pp. 139-140.

29. Hooton, "Where Did Man Originate?," Antiquity 1 (1927): 137-150.

30. Ibid., p. 147.

31. Ibid.

32. Ibid., p. 148.

33. Ibid., pp. 150, 148.

34. Ibid., p. 149.

35. Ibid.

36. Ibid., pp. 149-150.

37. Hooton, "Doubts and Suspicions Concerning Certain Functional Theories of Primate Evolution," Human Biology 2 (1930): 223-249.

38. Ibid., pp. 225-226; the classic exposition of the arboreal theory is contained in Sir Grafton Elliot Smith's 1912 "Address to the Anthropological Section (the Evolution of Man);" for Wood Jones' version see Frederic

Wood Jones, Arboreal Man (London: Edward Arnold, 1916).

39. Hooton, "Doubts and Suspicions," pp. 226-229.

40. Ibid., p. 224.

41. Elliot Smith was quite aware of the Lamarckian tone of his theory, and was unapologetic about it. Speaking of the "transformation of a Tarsoid into a monkey," he said,

> the exact determination of the sequence of developments whereby a highly complex series of voluntary coordinated movements came to be acquired and rendered automatic seems to imply that in some way the increasing ability to perform such actions by an intense effort of the will was transmitted hereditarily.

See Elliot Smith, The Evolution of Man: Essays 2nd ed. (London; 1927), 153.

Wood Jones, for his part, defended use inheritance and criticized the variation-selection model as insufficient to explain progressive evolution. See Wood Jones, Man's Place Among the Mammals (New York: Longmans, 1929), pp. 26-30.

42. Hooton, "Doubts and Suspicions," pp. 229-232.

43. Ibid., p. 234.

44. Ibid., p. 244. See also, Hooton, "Methods of Racial Analysis," Science, n.s. 63 (1926): 75-81.

45. Ibid., p. 249.

46. Ibid.

47. Ibid.

48. See, for example, Hammond,"Anthropology as a Weapon of Social Combat," and R.M. Young,"The Historiographic and Ideological Context of the Nineteenth Century Debate on Man's Place in Nature," pp. 344-438 in Young and M. Teich, Changing Perspectives in the History of Science (London: Heinemann, 1973); the relationship of scientific ideas to the belief systems prevalent in the general culture and/or within the social classes of a particular period is discussed from several angles in Barry Barnes and Steven S. Shapin, Natural Order:

Historical Studies of Scientific Culture (Beverly Hills and London: Sage, 1979).

49. Hooton, *Up From the Ape* (New York: Macmillan Company, 1931).

50. Ibid., p. 143.

51. Ibid., pp. 161-164.

52. Ibid., p. 135.

53. Ibid., p. 143.

54. Ibid., p. 113.

55. Ibid., p. 161.

56. Ibid.

57. Ibid., p. 115.

58. Ibid., p. 161.

59. Ibid., p. 164.

60. Ibid., p. 142.

61. Raymond Pearl, "The Constitutional Factors in the Breakdown of the Respiratory System," *Annals of Eugenics* 2 (1927): 1-24.

62. Hooton, *Up From the Ape,* pp. 277-278.

63. Ibid., 288.

64. See pp. 319-320 above.

65. Hooton, *Up From the Ape,* pp. 602-603.

66. Ibid., p. 603.

67. Ibid., p. 153, 294; the ebb and flow of scientific discussion on "encephalization" is discussed by H.J. Jerison, "Fossil Evidence of the Evolution of the Human Brain," *Annual Review of Anthropology* 4 (1975): 27-58. The pioneering works on the search for mathematical regularities in brain - body ratios had come well before Hooton began his work on *Up From the Apes* -- e.g. Eugene Dubois, "Sur le Rapport du Poids de L'Encéphale avec la

Grandeur du Corps chéz Mammiféres," Bull. Soc. Anthropol. Paris 8 (1897):337-376; L. Lapique,, "Sur la Relation du Poids de L'Encéphale au Poids du Corps," C.R. Soc. Biol 50 (1898): 62-63; and O. Snell, "Die Abhängigkeit hes Hirngewichtes von den Körpergewicht und den Geistigen Fähigkeiten," Arch. Psychiat. NervKrank, 23 (1891): 436-446. In fact, a careful analysis of the issue had been produced by the Dutch neuroanatomist C.U. Ariens Kappers just prior to the appearance of Hooton's book. See Ariens Kappers, The Evolution of the Nervous System (Harlem, 1929).

68. Hooton, Up From the Ape, pp. 151-152.

69. Ibid., pp. 165-167.

70. See the discussion of Symington in the MacCurdy chapter pp. 154-155.

71. Hooton, Up From the Ape, p. 294.

72. Frederick Tilney, The Brain From Ape To Man: with Chapters on the Reconstruction of Grey Matter in the Primate Brain Stem by Henry A. Riley (New York: P. Hoeber, 1928). Tilney's magnum opus (over 900 pages worth) had two major divisions; the first was an interesting attempt to estimate the proportions of the various cortical and subcortical regions in living primates by measuring the proportions of the nerve bundles in the brain stem that connect with the respective regions, while the second was a wholly conventional exercise in reading psychological capacities from the convolutional detail contained on brains and allegedly contained on the endocranial casts of fossils.

73. Hooton, Up From the Ape, pp. 163, 295.

74. Ibid., p. 308.

75. Ibid., p. 333.

76. Ibid.

77. Ibid., p. 294.

78. Davidson Black, "On the Discovery, Morphology and Environment of Sinanthropus Pekinensis ," Philosophical Transactions of the Royal Society of London, sec. B. 223 (1934): 57-120, was Black's full report, delivered in December 1932. Previous reports on the

"Sinanthropus" skull were Black," Sinanthropus Pekinensis: the Recovery of Further Fossil Remains of this Early Hominid from the Chou Kou Tien Deposits," Science 69: 674-676, and "On an Adolescent Skull of Sinanthropus Pekinensis in comparison with an Adult Skull of the Same Species and with Other Hominid Skulls, Fossil and Recent," Paleontologia Sinica, New Series D. 7 (1931): 1-144.

79. Noel T. Boaz, "History of American Paleoanthropological Research on Early Hominidae, 1925-1980," American Journal of Physical Anthropology 56 (1981): 397-405, pp. 399-400.

80. Hooton, Up From the Ape, p. 381.

81. Ibid., pp. 390, 571-583.

82. Ibid., p. 575.

83. Ibid., p. 335.

84. Ibid., p. 314.

85. Ibid., pp. 351-355.

86. Ibid., p. 462.

87. Ibid., pp. 334, 322-333.

88. See pp. 68-70 above.

89. Hooton, Up From the Ape, pp. 370-372.

90. Ibid., pp. 373-374.

91. Ibid., p. 375.

92. Ibid., pp. 578-579.

93. Ibid., pp. 402-403.

94. This was a fact that Hrdlička also noted, but of which he gave a contrasting interpretation. See pp. 250, 285 above

95. Hooton, Up From the Ape, p. 335.

96. See note 92 in the chapter on Hrdlička.

97. The classic statement of brain-led evolution is

also contained in the address given by Elliot Smith in 1912, "The Evolution of Man;" see note 38 above.

98. For a discussion of recapitulation, atavisms, etc. with a deep historical dimensions as well, consult Stephen J.Gould, Ontogeny and Phylogeny (Cambridge: Harvard University Press, 1977).

99. Ibid., pp. 167-206.

100. Hooton, Up From the Ape, 288; Hooton drew most of his information from Schultz, "Growth Studies on Primates Bearing Upon Man's Evolution," American Journal of Physical Anthropology , 7 (1924): 149-164, and "Studies on the Growth of the Gorilla and Other Higher Primates with Special Reference to the Fetus of Gorilla, Preserved in the Carnegie Museum," Memoirs of the Carnegie Museum 11 (1927): 1-87.

101. Hooton, Up From the Ape, pp. 228-233.

102. Ibid., pp. 233-238.

103. Ibid., p. 234 for this issue specifically.

104. Ibid., p. 241.

105. Ibid., p. 240.

106. Ibid., p. 244.

107. Ibid., p. 240.

108. Ibid.

109. A recent attempt to correlate reproductive strategies with other aspects of hominid behavioral and morphological evolution, and one that contains a very good bibliography is C. Owen Lovejoy, "The Origin of Man," Science 211 (1981): 341-350.

110. Hooton, Up From the Ape, p. 277.

111. Ibid., 288.

112. Ibid. 539; in an address eventually published in the book Twilight of Man (New York: G.P. Putnam's Sons, 1939), Hooton developed and expanded on these themes, which were then obviously matters of bitter controversy. See Hooton, "Noses, Knowledge, and Nostalgia -- the Marks

of a Chosen People," pp. 227-250.

113. Hooton, Up From the Ape, pp. 523-571.

114. Ibid., p. 501.

115. Ibid., p. 546.

116. It is interesting to note here the way in which Hooton drew opposite inferences from the same notion of unstimulating environments to the ones that Osborn drew in his discussions of racial psychology. See note 184 in the chapter on Osborn.

117. Hooton, Up From the Ape, p. 594.

118. Ibid., pp. 501-502.

119. Ibid., p. 595.

120. Hooton, Apes, Men and Morons (New York: G.P. Putnam's Sons, 1937).

121. Ibid., p. 20.

122. Ibid., pp. 21-22.

123. Ibid., p. 28.

124. See pp. 211-214 above.

125. Hooton, Apes, Men and Morons, pp. 46-54; quotation from p. 50.

126. Hooton, Twilight of Man (New York: G.P. Putnam's Sons, 1939), pp. 256, 258.

127. Ibid., p. 256.

128. Ibid., p. 258. It is interesting to note the new, and pessimistic context within which the older theory was reasserted. The appearance of aggressive dictatorships in Italy and Germany,and to some degree the Depression and New Deal welfare state, had apparently filled Hooton with foreboding about the human future. A self-conscious and unapologetic elitist, he attributed human cultural and technological progress largely to the achievements of biologically gifted individuals; the general capacity of human beings to profit from these advances, even after the ignorant mass had done its best

to stifle those responsible for creating them, had, he believed, created new dangers to further human progress. Modern technology had made the cultural transmission of knowledge through education more important than the promotion of biological intelligence in the population. Distorted modern ideals had also provided weapons by which "predacious" demagogues could transform mass democracy into dictatorship. In the absence of strong natural selection from which human culture had largely freed the species, only a conscious commitment to maintaining and improving the biological endowment of man, Hooton thought, could forestall further deterioration of modern social conditions. He had ccome to wonder whether human initiative had not created conditions perilous to its own continued survival. His full argument is contained in the essay in Twilight of Man entitled "Anthropological Prospect of the Survival of Human Liberty," pp. 251-283.

129. Hooton, Apes, Men, and Morons, p. 256.

130. Ibid., 112.

131. Franz Weidenreich, "The Dentition of Sinanthropus Pekinensis; A Comparative Odontography of the Hominids," Paleontolgia Sinica, New Ser. D. no. 1, whole ser. 101 (Peiping: Geological Society of China, 1937).

132. Hooton, Apes, Men and Morons, pp. 97-98.

133. Ibid., p. 100.

134. Ibid.

135. Summarizd in P. Teilhard de Chardin and W.C. Pei, "The Lithic Industry of the Sinanthropus deposits in Choukoutien," Bull. Geol. Society China, 13 (1932): 315-358.

136. Hooton, Apes, Men and Morons, p. 95.

137. Hooton, "Review of L.S.B. Leakey, The Stone Age Races of Kenya ," American Anthropologist, n.s. 37 (1935): 681-684, p. 684.

138. Hooton, Apes, Men and Morons, p. 97.

139. Hooton, Why Men Behave Like Apes and Vice Versa: or, Body and Behavior (Princeton: Princeton University Press, 1940).

140. Hooton, Why Men Behave, pp. 79-80.

141. Ibid., p. 71.

142. Ibid., pp. 77-78.

143. We have of course seen it repeatedly in the work of MacCurdy; see pp. 151-152 above. Also, it should be noted that Harvard's resident expert on the Asian Paleolithic during the 1940s, Hallam J. Movius, Jr., was inclined to accept Hooton's theory that the two cultural traditions had been produced by different racial lines. See Movius' important monograph, "Early Man and Pleistocene Stratigraphy in Southern and Eastern Asia," Peabody Museum Papers 19 (1944): 102.

144. Hooton, Why Men Behave, p. 78.

145. See pp. 113-122 above.

146. See the chapter on Hrdlička, p. 287.

147. Hooton, Why Men Behave, p. 71.

148. G.M. Morant, "The Form of the Swanscombe Skull," Journal of the Royal Anthropological Institute of Great Britain 68 (1938): 67-98, p. 97.

149. Hooton, Apes, Men, and Morons, p. 144.

150. Hooton, Why Men Behave, pp. 102-148.

151. Ibid., pp. 85-102.

152. Hooton, Apes, Men and Morons, p. 105.

153. Ibid., p. 107.

154. Ibid., p. 108.

155. Ibid., p. 111.

156. Ibid., p. 112.

157. Ibid., p. 109.

158. Ibid., pp. 112-113.

159. Ibid., p. 113.

160. Ibid., p. 114.

161. See p. 364 above.

162. Hooton, Apes, Men and Morons, p. 117.

163. See for example pp. 545-571 below.

164. Hooton, Apes, Men and Morons, pp. 118-119.

165. Ibid., pp. 119-122.

166. Hooton, Man's Poor Relations (Garden City: Doubleday, Doran and Comany, 1942), p. xi.

167. Though not clearly expressed in this book, the idea had been repeated explicitly in the collection of essays produced just before Man's Poor Relations -- Why Men Behave Like Apes, and Vice Versa ; see pp. 54-55 in the latter work. There, Hooton also generalized the proposition further. It would be easy, he said, to point out inumerable traits of social, technological and ideational behavior unique to man; still, he concluded, "for the most part these peculiarities arise from extensions of the tool-making capacity or are concomitants of articulate speech. Both speech and the use of tools, of course, stem back to the size and intricacy of the human brain" (p. 55). And to underline this conclusion he added the following: "Qualitatively, it would seem, the distinction between human and other primate behavior is tenuous, although quantitatively vast. On the whole, the degree of difference between man and the ape seems smaller in function and behavior than in anatomy and morphology" (p. 56).

168. Hooton, Man's Poor Relations, p. xi. Hooton's hesitance about the idea of a "large-bodied" brachiating ancestor was made more explicit along with the reasons for that hesitance in the revised edition of Up From the Ape. See the discussion of that work below.

169. See pp. 359-360 above.

170. Hooton, Man's Poor Relations, pp. 101-105, 131-136.

171. Ibid., pp. 48-51, 55.

172. Ibid., pp. 257-260; the experiments Hooton was summarizing were described and evaluated in Heinrich

Klüver, Behavior Mechanisms in Monkeys (Chicago: University of Chicago Press, 1938).

173. Hooton, Man's Poor Relations, pp. 157-158; see Carpenter's original monograph, A Field Study in Siam of the Behavior and Social Relations of the Gibbon(Hylobates lar) (Baltimore: Johns Hopkins University Press, 1941).

174. Hooton, Man's Poor Relations, p. 80.

175. Ibid., p. 130.

176. Ibid., pp. 130-131.

177. Ibid., p. 54.

178. Ibid., pp. 323-324.

179. Solly Zuckerman, The Social Life of Monkeys and Apes (New York: Harcourt, Brace and Company, 1932).

180. Hooton, Man's Poor Relations, pp. 325-327.

181. Ibid., p. 327.

182. Ibid., p. 329.

183. Ibid.

184. Ibid., p. 332.

185. Ibid., p. 331; Keith would also feature the "inbreeding" or reproductive isolation supposedly induced by territoriality in A New Theory of Human Evolution (London: Watts and Company, 1948).

186. Hooton, Man's Poor Relations, pp. 331-332.

187. Ibid., pp. 342-353.

188. Ibid., pp. 389-390.

189. Ibid., pp. 390-391.

190. Ibid., p. 390.

191. See Charles Loring Brace, "Tales of the Phylogenetic Woods: The Evolution and Significance of Evolutionary Trees," American Journal of Physical Anthropology 56 (1981): 411-429.

192. Consult Harry L. Shapiro, Peking Man (New York: Simon and Schuster, 1974), for the facts surrounding the mysterious disappearance.

193. Franz Weidenreich, "The Skull of Sinanthropus Pekinensis: A Comparative Study on a Primitive Hominid Skull," Paleontologia Sinica, Whole Series no. 127, New Series D.no. 10 (1943), and Theodore McCown and Arthur Keith, The Stone Age of Mt. Carmel, v. 2 (Oxford: The Clarendon Press, 1939).

194. Franz Weidenreich, "Giant Early Man from Java and South China," Anthropological Papers of the American Museum of Natural History, 40, pt. 1 (1945), contains material on these creatures, as well as the comparative sections of "The Skull of Sinanthropus."

195. Theodosius Dobzhansky, "On Species and Races of Living and Fossil Man," American Journal of Physical Anthropology, n.s. 2 (1944): 251-265.

196. Hooton, Up From the Ape, Revised Edition (New York: Macmillan, 1946).

197. Ibid., pp. 146-152, 161-162. A glaring omission from this part of the book, obviously, was the well-known critique of cerebral localization theory propounded by the American scientist Karl Lashley, a critique which the latter had summarized in his classic Brain Mechanisms and Intelligence (Chicago: University of Chicago Press, 1929). Hooton had missed Lashley's critique the first time around, but it was quite surprising that he did not take notice of it in the revision, if only to dismiss Lashley's work, which had been done on rats, as not relevant to the higher primates.

198. Hooton, Up From the Ape, rev. ed., pp. 141-142.

199. Ibid., pp. 158-160.

200. See p. 412 above.

201. Hooton, Up From the Ape, rev. ed., p. 139.

202. Ibid., pp. 159-162.

203. See pp. 352-353 above.

204. Hooton, Up From the Ape, rev. ed., pp. 133-135.

205. Ibid., pp. 130-135, the quotation is from pp. 133-134.The general shift in the period against Miocene brachiators, which actually affected Keith as well as many others, is discussed in Fleagle and Jungers, "Fifty Years of Higher Primate Phylogeny," pp. 196-201. The key works by Schultz and Straus are Schultz, "Characters Common to Higher Primates and Characters Specific to Man," Quarterly Review of Biology 11 (1936): 259-283, 425-455, and Straus, "The Posture of the Great Ape Hand in Locomotion, and its Phylogenetic Implications," American Journal of Physical Anthropology 27 (1940): 199-207, and "The Phylogeny of the Human Forearm Extensors," Human Biology 13 (1941): 23-50, 203-238.

206. See Thomas Kuhn, "Structure of Scientific Revolutions," pp. 77-91.

207. Boaz, "Research on Early Hominidae," pp. 401-402; Wilfred E. Le Gros Clark, Man-Apes or Ape-Men? (New York: Holt, Rinehart and Winston, 1967), provides an account of the early australopithecine discoveries and interpretations, as well as a discussion of that author's "conversion." On the australopithecine teeth, and on Gregory's ideas about them, see pp. 590-605 below.

208. An early instance of this trend was the article by Loren Eiseley, "Some Paleontological Inferences as to the Life-Habits of the Australopithecines", Science 98 (1943): 61-62. A sign of how far things had come by 1950 was Ernst Mayr's proposal that the australopithecines should be included in the genus Homo , a proposal made in his contribution to an extremely important Cold Spring Harbor Symposium on Quantitative Biology devoted to the evolution of humanity. Mayr's suggestion was not and has not since been adopted by many other biologists, but it is a clear indication of the way in which post war scientists became more willing to overlook supposed "apelike" characters like the small brains of the South African "man-apes." See Mayr,"Taxonomic Categories in Fossil Hominids," Cold Spring Harbor Symposia on Quantitative Biology 15 (1950): 103-118.

209. Hooton, Up From the Ape, rev. ed., p. 282.

210. Ibid., p. 283.

211. Ibid.

212. See, for example, Donald C. Johanson and T.D. White, "A Systematic Assessment of Early African Hominids," Science 203 (1979): 321-330.

213. Hooton, Up From the Ape, rev. ed., p. 288.

214. Weidenreich, "The Skull of Sinanthropus," pp. 216-220.

215. Hooton, Up From the Ape, rev. ed., p. 316.

216. See note 81 above.

217. By Weidenreich's count, parts of 14 individual skulls of "Sinanthropus" had been found by 1941, and fragments of another half dozen representatives of "Pithecanthropus". See Weidenreich, "The Skull of Sinanthropus", pp. 5-7. It should also be pointed out that Weidenreich's opposition to Piltdown was based on what he considered to be strong theoretical arguments as well as the overwhelming weight of the evidence; in Weidenreich's conception of hominid emergence the achievement of upright posture was a precondition for the later growth of the brain to modern human proportions, since the former had made possible the reduction of the face and bending downward of the jaw above which the globular human skull vault could be built. Thus, a protruding face and jaw and rounded braincase seemed a paradoxical and mechanically tenuous combination. See Weidenreich, "The Phylogenetic Development of Man and General Theories on Evolution," Bulletin of the Geological Society of China 19 (1939): 76-92, pp. 86-87.

218. Hunt, "The Old Physical Anthropology," pp. 343-344.

219. Hooton, Up From the Ape, rev. ed., p. 363.

220. Ibid., p. 298.

221. Ibid.

222. Ibid.

223. Ibid., p. 305.

224. See pp. 392 and 396-397 above.

225. Hooton, _Up From the Ape,_ rev. ed., p. 305.

226. Ibid., p. 348.

227. Ibid., p. 346.

228. Whether this feature is peculiar to Neanderthaloids is still an issue in paleoanthropology. A recent analysis of the phenomenon is Erik Trinkaus and Marjorie LeMay, "Occipital Bunning Among Later Pleistocene Hominids," _American Journal of Physical Anthropology,_ 57 (1982): 27-35.

229. Hooton, _Up From the Ape,_ rev. ed., pp. 333-335.

230. Ibid., p. 338.

231. Ibid.

232. Ibid.

233. Ibid. A similar, and even more strongly worded, criticism, on genetic grounds was made by M.F. Ashley Montagu in his review of Keith and McCown's work. See M.F. Ashley Montagu, "Review of McCown and Keith, _The Stone Age of Mt. Carmel, v.2,_ " _American Anthropologist_ 42 (1940): 518-522.

234. Reginald Ruggles Gates, _Human Ancestry From a Genetical Point of View_ (Cambridge: Harvard University Press, 1948).

235. Arthur Keith, _A New Theory of Human Evolution_ (London: Watts and Company, 1948).

236. Arthur Keith, _An Autobiography_ (London: Watts and Company, 1950), pp. 629-631.

237. Hooton, "Review: Sir Arthur Keith's _A New Theory of Human Evolution_ ," _Antiquity_ 23 (1949): 126-128, p. 127.

238. Sherwood L. Washburn, "The Piltdown Hoax," _American Anthropologist_ 55 (1953): 759-762.

239. Ibid., p. 761.

240. Ibid., p. 762.

241. Ibid., 761.

242. Ibid.

243. Hooton, "Comments on the Piltdown Affair," American Anthroppologist 56 (1954): 287-289.

244. Ibid., p. 287.

245. Ibid.

246. Ibid., p. 288.

247. Ibid.

248. Ibid., p. 289.

249. Ibid., p. 288.

250. Hooton, "The Importance of Primate Studies in Anthropology," Human Biology 26 (1954): 179-188.

251. Ibid., p. 181.

252. Ibid., p. 185. Dart's theory and his supporting evidence are contained in Raymond Dart, "The Predatory Implemental Technique of Australopithecus ," American Journal of Physical Anthropology, n.s. 7 (1949): 1-38.

253. Hooton, "The Importance of Primate Studies," p. 182.

254. Ibid., pp. 185-186.

Chapter V

1. Edwin H. Colbert, "William King Gregory, 1976-1970," Biographical Memoirs of the National Academy of Sciences 46 (1975): 92-193, p. 92.

2. Ibid., pp. 93-94.

3. Ibid., pp. 95, 98-99.

4. Sherwood L. Washburn, "Wiliam King Gregory,

1976-1970," American Journal of Physical Anthropology 56 (1981): 393-395, p. 394.

5. Colbert, "William King Gregory," p. 95.

6. Ibid., pp. 103-104.

7. See pp. 545-560 below.

8. Colbert, "Wiliam King Gregory," p. 105.

9. As we shall see in what follows, Gregory understood the term "brachiation" in the general sense in which Sir Arthur Keith had defined it early in this century -- i.e. as locomotion through the trees by means of arm-swinging. Gregory did not differentiate among the specific styles of brachiation adopted by various anthropoids. Careful biomechanical study of the latter issue did not begin until after World War II. See e.g. Virginia Avis, "Brachiation: the Crucial Issue in Man's Ancestry," Southwestern Journal of Anthropology 18 (1962): 119-148.

10. See pp. 614-615 below.

11. This tendency was of course most obvious in Hooton's writings, but it can be seen as well in the general willingness of students of fossil hominids to accept generic status for every new "type" discovered.

12. This is a point also stressed by Noel T. Boaz, "History of American Paleoanthropological Research on Early Hominidae, 1925-1980," American Journal of Physical Anthropology 56 (1981): 397-405, p. 400.

13. William K. Gregory, "Genetics versus Paleontology," American Naturalist 51 (1917): 622-635, p. 623.

14. See pp. 88-94 above.

15. William K. Gregory, "The Orders of Mammals. I. Typical Stages in the History of the Ordinal Classification of Mammals. II. Genetic Relations of the Mammalian Orders: with a Discussion of the Origin of the Mammalia and of the Problem of the Auditory Ossicles," Bulletin of the American Museum of Natural History 27 (1910): 1-524. This work was the published version Gregory's doctoral dissertation,and as Washburn rightly notes, "There must be very few Ph.D. theses of comparable

size, importance and originiality!" S.L. Washburn, "W.K. Gregory," p. 394.

16. Gregory, "The Orders of Mammals," p. 106.

17. Ibid.

18. Ibid., p. 107.

19. Gregory, "Genetics _versus_ Paleontology," p. 624.

20. Gregory, "The Orders of Mammals," p. 112.

21. Ibid., p. 111.

22. Ibid., p. 112,111.

23. W.K. Gregory, "Locomotive Adaptations in Fishes Illustrating 'Habitus' and 'Heritage', _Annals of the New York Academy of Science_ 22 (1914): 267-268, p. 267.

24. Gregory, "Locomotive Adaptations," p. 268.

25. See pp. 96-97 above.

26. Henry Fairfield Osborn, "The Angulation of the Limbs of Proboscidea, Dinocerata and other Quadrupeds in Relation to Weight," _American Naturalist_ 34 (1900): 89-94.

27. W.K. Gregory, "Notes on the Principles of Quadrupedal Locomotion and on the Mechanism of the Limbs in Hoofed Animals," _Annals of the New York Academy of Science_ 22 (1912): 267-294.

28. The major piece of research that Gregory did in this line was on the hominoid humerus during his debates with Osborn; see Gregory, "Were the Ancestors of Man Primitive Brachiators?," and "The Upright Posture of Man: a Review of its Origin and Evolution," _Proceedings of the American Philosophical Society_ 67 (1928): 129-150 and 339-376 respectively.

29. Dudley J. Morton, "Evolution of the Human Foot. I.," _American Journal of Physical Anthropology_ 5 (1922): 305-336, and "Evolution of the Human Foot. II.," _American Journal of Physical Anthropology_ 7 (1924): 1-52, were the first of these; Morton's general conclusions on the problem were contained in "Evolution of Man's Erect Posture (Preliminary Report)," _Journal of Morphology and_

Physiology 43 (1926): 147-179, and "Human Origin: Correlation of Previous Studies on Primate Feet and Posture with Other Morphological Evidence," American Journal of Physical Anthropology 10 (1927): 173-203.

30. W.K. Gregory, "Studies on the Evolution of the Primates. I. The Cope-Osborn 'Theory of Trituberculy' and Ancestral Molar Patterns of the Primates," Bulletin of the American Museum of Natural History 35 (1916): 239-257, and "Studies on the Evolution of the Primates. II. Phylogeny of Recent and Extinct Anthropoids, with Special Reference to the Origin of Man," Bulletin of the American Museum of Natural History 35 (1916): 258-355.

31. Gregory, "Evolution of the Primates," pp. 239-248 restates the theory of "trituberculy" with some critical comments; later Gregory devoted an entire monograph to refining and updating the theory, especially in regard to the origin of the tritubercular pattern. See Gregory, "A Half Century of Trituberculy. The Cope-Osborn Theory of Dental Evolution, with a Revised Summary of Molar Evolution from Fish to Man," Proceedings of the American Philosophical Society 73 (1934): 169-317.

32. Henry Fairfield Osborn, Evolution of Mammalian Molar Teeth to and from the Triangular Type, Including Collected and Revised Researches on Trituberculy and New Sections on the Forms and Homologies of the Molar Teeth in the Different Orders of Mammals, edited by W.K. Gregory (New York: Macmillan, 1907).

33. Gregory, "Evolution of the Primates," p. 254.

34. Ibid., pp. 263-265.

35. Ibid., pp. 296-298.

36. Ibid., pp. 257, 293.

37. Ibid., p. 276; there is also a similar comment on p. 340.

38. Ibid., p. 341.

39. Though the similarities in the dentition that Gregory was mainly concerned with made this analogy a reasonable one, it has been strongly challenged by more recent students of the dryopithecine group. David Pilbeam, for example, states that "the early Miocene forms, rather than being sampled from a low diversity group that was

morphologically, behaviourally, and ecologically like modern apes, probably represent a diverse radiation of truly primitive species, cranially, dentally and post-cranially distinct from living apes ... Predominantly forest arboreal forms, their closest ecological analogues are best seen as monkeys rather than apes," David Pilbeam, "Major Trends in Humaan Evolution," pp. 261-285 in Lars Konig Konigson ed., Current Argument on Early Man (Oxford: Pergamon Press, 1980), p. 271.

40. Gregory, "Evolution of the Primates," pp. 277-278. The addition of the phrase "and for fighting" was critical in the case of the canine teeth, since Gregory was well aware that the females of each pongid species seemed able to eat efficiently with much smaller canines than those possessed by their male counterparts.

41. Ibid., pp. 275-277; the quote is from p. 275.

42. Ibid., pp. 278-279.

43. Ibid., p. 279.

44. Ibid., p. 334.

45. Ibid., p. 333; a similar idea is also expressed on p. 277.

46. Ibid., p. 277.

47. Ibid., p. 331.

48. Ibid., p. 332-333.

49. Ibid., p. 327.

50. Ibid., p. 342; the implication of close genetic affinity between Sivapithecus and hominids here seems to reflect support on Gregory's part for the views that Guy Pilgrim, Sivapithecus' discoverer, expresed on this fossil in 1915. See Guy E. Pilgrim, "New Siwalik Primates and Their Bearing on the Question of the Evolution of Man and the Anthropoidea," Records of the Geological Survey of India, 40 (1910): 1-74. In later discussions of the dryopithecines Gregory would become less definite about Sivapithecus' position in relation to the hominid line.

51. Gregory, "Evolution of the Primates," pp. 342-343.

52. Ibid., p. 343.

53. Ibid., 343; it is interesting to note that the language in this passage attributes to early hominids the same pattern of cultural learning that MacCurdy hypothesized. See the chapter on MacCurdy, p. 179-182.

54. Gregory, "Evolution of the Primates," p. 321.

55. Ibid., 343-344.

56. Ibid., p. 344.

57. Ibid.

58. Ibid., p. 320.

59. Ibid., pp. 321-322.

60. Ibid., pp. 321-322, 344.

61. Gregory, "Evolution of the Primates," p. 322; the "killer ape" hypothesis of hominid origins, in a form very similar to the one employed by Gregory, has continued to be important in post World War II paleoanthropology, though it has been attacked quite vigorously in recent years. See, for example Johanson and White, "A Systematic Assessment of Early African Hominids;" and Matt Cartmill, "Four Legs Good: Two Legs Bad!"

62. Gregory, "Evolution of the Primates," pp. 322-323.

63. Ibid., pp. 323-324; Sir Arthur Keith, The Antiquity of Man, pp. 118-130.

64. Gregory, "Evolution of the Primates," p. 325.

65. Ibid., p. 326; See pp. 256-259 above.

66. See pp. 252-253 above.

67. Gregory, "Evolution of the Primates," pp. 327-328.

68. F. Clark Howell, "The Evolutionary Significance of Variation and Varieties of 'Neanderthal' Man."

69. Gregory, "Evolution of the Primates," 328; note how in a general way Gregory's idea of "widely separated

times and places of origin" for emergence of Neanderthaloids into Homo sapiens foreshadows theories later developed in detail by Franz Weidenreich. See for example, Weidenreich, "The 'Neanderthal Man' and the Ancestors of 'Homo Sapiens,'" American Anthropologist, n.s. 45 (1943): 39-48, and "Facts and Speculations Concerning the Origin of Homo sapiens, " American Anthropologist, n.s. 49 (1947): 187-203. It is not surprising, then that late in his career Gregory would show a great deal of favorable interest in Weidenreich's conception of human evolution. See pp. 623-626 below.

70. Gregory, "Evolution of the Primates," p. 323.

71. Ibid., p. 316; Gregory's support for Miller was even more emphatic in the former's own contribution to the early stages of the Piltdown debate. Thus, in 1916 Gregory reported his own analysis of photographs of the Piltdown mandible and pointed out four ways in which the molar teeth were more like worn chimpanzee or gorilla teeth than they were like any human teeth he had examined. While to him this was clear evidence that Miller was right about "Pan vetus" Gregory conceded that disagreement would likely continue, because "while the resemblances and differences are in a sense objective phenomena, the cognition, or perception, of generic identity is an individual experience, like the perception of truths and abstract propositions." Gregory, "Note on the Molar Teeth of the Piltdown Mandible," American Anthropologist, n.s. 18 (1916): 384-387, p. 385. In an article published jointly with W.D. Matthew and C.R. Eastman in the same year, Gregory went even further in joining Miller's attack on the English defenders of "Eoanthropus"when the authors asserted that

> it is necessary here to distinguish between the concepts of resemblance and identity. The Piltdown jaw is not simply a jaw similar in adaptive specialization to that of an ape, it is a jaw identical with that of the chimpanzee in every particular. The skull is not merely similar in brain case to that of man, it is the skull of Homo in every particular. ... Such a combination as this, with its utter lack of blending, correlation or coordination of interrelated parts ... is without parallel and it not reasonably possible.

See Gregory, Matthew and Eastman, "Recent Progress in Vertebrate Paleontology," Science 43 (1916): 103-110, pp. 107-108.

72. Gregory, "Evolution of the Primates," p. 316.

73. Ibid., p. 323.

74. See p. 492 above.

75. See the chapter on Osborn p. 86 for a similar show of reluctance.

76. See below, pp. 527-533.

77. The latter line of work achieved its best-known manifestation in the book Our Face from Fish to Man (New York: Putnam's, 1929).

78. Gregory, "On the Structure and Relations of Notharctus, an American Eocene Primate. Studies on the Evolution of the Primates. Part III," Memoirs of the American Museum of Natural History 3 (1920): 49-243; and Gregory, "The Origin and Evolution of the Human Dentition. A Paleontological Review. Parts 1-4," Journal of Dental Research 2 (1920): 89-183, 215-283, 357-427, 607-717, "The Origin and Evolution of the Human Dentition. Part 5," Ibid. 3 (1921): 87-228.

79. Gregory, "Evolution of the Human Dentition, Part 5," pp. 118-124, for example presents a verbatim repetition of passages form "Evolution of the Primates;" there are similar, though briefer instances elsewhere, especially in interpretive sections of Part 5.

80. The first published report of the "Piltdown II" fragments was Arthur Smith Woodward, "Fourth Note on the Piltdown Gravel, with Evidence of a Second Skull of Eoanthropus Dawsoni," Quarterly Journal of the Geological Society of London 73 (1917):1-10.

81. Gregory, "Evolution of the Human Dentition. Part 4," p. 689.

82. Ibid., p. 698.

83. Gregory, "Evolution of the Primates," p. 343.

84. Gregory, "Evolution of the Human Dentition. Part 5," p. 215. See pp. 105-107 above for the variant of this idea developed by Joseph Barrell.

85. Gregory, "Evolution of the Human Dentition.

Part 5," p. 111.

86. Ibid., pp. 218, 139.

87. Ibid., pp. 142-146; the data was from casts and photos rather than from analysis of the original specimens.

88. Ibid., p. 157.

89. Ibid., pp. 140-141.

90. Ibid., p. 140.

91. Ibid.

92. Ibid.

93. Ibid., p. 156.

94. Ibid., p. 218.

95. Ibid., p. 112.

96. Ibid.

97. Ibid., p. 110.

98. Gregory and Milo Hellman, "The Dentition of Dryopithecus and the Origin of Man," Anthropological Papers of the American Museum of Natural History 28, part 1 (1926): 1-123. Beginning in 1923 Gregory produced joint articles regularly on matters relating to primate dental morphology. Though I have not analyzed the distribution of labor in this partnership, the great similarity in both style and intellectual content between these collaborative works and solo efforts by Gregory indicate clearly that Gregory was the leader in matters of interpretation.

99. See Hrdlička, "New Data on the Teeth of Early Man and Certain Fossil European Apes," American Journal of Physical Anthropology 7 (1924): 109-132. Interestingly Hrdlicka did not develop specific phylogenetic conclusions from this research on dryopithecine teeth. As Spencer points out, the critical question for Hrdlička was how dietary and technological changes had operated on later stages of human evolution. See Spencer, "Aleš Hrdlička, M.D.," p. 480. Remane's data was reported in Adolf Remane, "Beiträge zum Morphologie des Anthropoidengebisses," Archiv für Naturgeschichte, 87

Jahrg., Abteil A, Heft 11 (1921): 1-179.

100. That Gregory resented Hrdlicka's failure to give adequate recognition for his work on the "*Dryopithecus* pattern" was made evident in a letter that he wrote to the latter in 1923. See Spencer, "Aleš Hrdlička, M.D.," pp. 539-542.

101. Gregory and Hellman, "Dentition of *Dryopithecus*," p. 31.

102. Ibid.

103. Changes in scale were necessary, however, to make the various fragments combine harmoniously (which of course introduced a major source of uncertainty, since character differences correlated with body size would have been ignored). Gregory and Hellman, "Dentition of *Dryopithecus*," p. 32.

104. Ibid., p. 37.

105. Ibid., pp. 110-111.

106. Ibid., p. 111.

107. Ibid.

108. Ibid., p. 110.

109. Ibid., p. 111.

110. See Ibid., plate 25.

111. See p. 484 above, and Gregory, "Evolution of the Human Dentition. Part 4," pp. 656-658.

112. Gregory and Hellman, "Dentition of *Dryopithecus*," p. 84. Interestingly, this judgement on *Sivapithecus* represented a return to a position that Gregory had staked out in 1915 (Gregory, "Is *Sivapithecus Pilgrim* an Ancestor of Man?," *Science* 42 (1915): 341-342). In addition, the willingness to divide the dryopithecines into eastern and western branches implied a certain hesitance about endorsing a strong version of the central Asian theory of higher primate dispersal. This is not to say that Gregory became unsympathetic to more moderate versions of the theory like those of Davidson Black (see Gregory, "Did Man Originate in Central Asia?," *Scientific Monthly* 24 (1927): 385-401). Still, Osborn's "Pro-Dawn

man" and the problems encountered over the so-called "Hesperopithecus" fossil had apparently made Gregory cautious about rigid adherence to the central Asian scheme. When the first "Hesperopithecus" fragment, a single, worn tooth from a Pliocene deposit in Kansas, came to light, Osborn had pronounced it an anthropoid second molar (Osborn, " Hesperopithecus, the First Anthropoid Primate Found in America," Science 55 (1922): 463-465). Gregory had seconded this judgement, and even ventured the opinion that "Hesperopithecus" was one of the "lower Pliocene survivors" of the dryopithecine radiation; both Osborn and Gregory took this as confirmation of the idea that the higher primates had indeed spread outward in several directions from a central Asian center (Gregory and Hellman, "Notes on the Type of Hesperopithecus Haroldcookii Osborn," American Museum Novitiates no. 53 (1923): 13). Erecting hypotheses of this importance on the basis of such fragmentary evidence was criticized, and Gregory undertook to defend himself (Gregory and Hellman, "Further Notes on the Molars of Hesperopithecus and Pithecanthropus," Bulletin of the American Museum of Natural History, 48 (1923): 509-526). Continued exploration of the "Hesperopithecus" deposits produced increasingly doleful results -- it became clear that a previously known genus of extinct peccary possessed upper premolars which, when worn down, matched the type specimen of "Hesperopithecus" very closely. Gregory conceded the issue formally in 1927, but the problem must have been on his mind when the "Dentition of Dryopithecus" was being prepared (see Gregory, " Hesperopithecus Apparently not an Ape Nor a Man," Science 66 (1927): 579-581.

113. Gregory and Hellman, "Dentition of Dryopithecus ," p. 83.

114. Gregory, "The Dawn Man of Piltdown, England," American Museum Journal 14 (1914): 188-200, p. 199.

115. Ibid., p. 199.

116. Ibid., p. 200.

117. Ibid.

118. Gregory, "Evolution of the Primates," p. 326; Marcellin Boule, "L'homme Fossile de La Chapelle-aux-Saints," Annales de Paléontologie 7 (1912): 85-192, ibid. 8 (1913): 1-67.

119. Gregory, "Evolution of the Primates," p. 347;

Boule, "La Paléontologie Humaine en Angleterre," L'Anthropologie 26, no. 1-2 (1915): 1-68. Boule continued to doubt the association of the Piltdown skull and braincase as well as to hypothesize the future discovery of a "true Eoanthropus" even after the discovery of the Piltdown II specimens. See Boule, Fossil Men: Elements of Human Paleontology, 2nd edition (London, 1923), pp. 174, 450.

120. Gregory, "Evolution of the Primates," pp. 347-348.

121. Ibid., p. 322.

122. Ibid., p. 333.

123. Ibid., p. 334.

124. Ibid.

125. Ibid.

126. Ibid.

127. Ibid., p. 332.

128. See the chapter on Osborn, pp. 88-91.

129. Gregory, "Evolution of the Primates," pp. 327, 335.

130. Gregory, "Evolution of the Human Dentition. Part 5," p. 110, 109.

131. Ibid., p. 96.

132. Ibid., p. 113, 117.

133. Ibid., p. 110.

134. Ibid.

135. Ibid., p. 117. The emphasis is Gregory's own.

136. Ibid., p. 114.

137. Ibid., p. 112.

138. Ibid., p. 94.

139. Sir Arthur Keith, "Review of 'Origin and Evolution of the Human Dentition' by W.K. Gregory," Nature 110 (1922): 834-836, p. 835.

140. Keith, "Review of Gregory," p. 835.

141. On Morton, see note 29 above; on Hrdlicka, note 99 above; Tilney's work is contained in the large monograph, The Brain from Ape to Man (New York: P. Hoeber, 1928). Concerning Schultz, see A.H. Schultz, "Growth Studies on Primates Bearing Upon Man's Evolution," American Journal of Physical Anthropology 7 (1924): 149-164.

142. For example, Sir Arthur Keith, "Man's Posture: Its Evolution and Disorders," British Medical Journal 1 (1923): 451-454, 499-502, 545-548, 589-590, 624-262, 669-672, and Concerning Man's Origin (London: Watts, 1927); Grafton Elliot Smith, The Evolution of Man (London: Oxford University Press, 1924). Frederick Wood Jones, The Ancestry of Man (Brisbane: Gillies, 1923), and Man's Place Among the Mammals (London: Edward Arnold, 1929).

143. Gregory, "Evolution of the Human Dentition," p. 111.

144. Gregory, "The Biogenetic Law and the Skull Form of Primitive Man," American Journal of Physical Anthropology 8 (1925): 373-378; it should be noted that Gregory was not unique in his dissatisfaction with the venerable idea of recapitulation. Attacks on it were apparently quite common in the years after 1900; see Stephen J.Gould, Ontogeny and Phylogeny, p. 202-206.

145. Gregory, "Biogenetic Law," p. 375.

146. Ibid., p. 376-377; the problem of man's "bulging forehead" and its resemblance to that of fetal apes raises an important aspect of the relationship between ontogeny and phylogeny that Gregory did not discuss in his 1925 critique of recapitulation --i.e. the theory of"neoteny", which is also called "fetalization" or "paedomorphosis". The germ of this theory is the idea that the process of evolution can proceed by the retention or intensification in adult descendants of the fetal or juvenile characters of ancestors. This inverse of the biogenetic law had been applied extensively to man in the decade prior to 1925 by the Dutch embryologist Ludwig Bolk and in fact Gregory himself had made use of it in the "Dentition of Dryopithecus ". In the course of speculating

about the evolutionary origins of the divergent canine forms of great apes and humans, Gregory had argued that the moderately robust canines of the dyopithecines could have been ancestral to both. Thus, the canine of male apes could have resulted from extending the growth process until it produced a hyper-robust "gerontomorphic" form, while the reduced proportions of root and crown in humans could have emerged from a truncated growth process and the retention of an "infantilized" canine form (p. 93). While embryonic here, Gregory's concern with the fetalization theory would develop into an important theme in several of his subsequent writings on human evolution. Regarding Bolk, see Gould, Ontogeny and Phylogeny, pp. 356-362.

147. Gregory, "Biogenetic Law," p. 378.

148. Ibid.

149. See pp. 73-90 above.

150. H.F. Osborn, The Hall of the Age of Man. American Museum of Natural History Guide Leaflet Series, no. 52. 3rd. edition (New York: American Museum of Natural History, 1925), edited by W.K. Gregory; see pp. 11, 6.

151. Osborn, Age of Man, p. 6n.

152. Osborn, Age of Man, p. 36-45. A significant area of agreement between Gregory and Osborn is also revealed in this appendix, since it underlined the former's acceptance of polyphyly among Plio-Pleistocene hominids. Gregory's version of "man's family tree" in the pamphlet thus went as far as Osborn's in putting all forms of fossil humans except the "Cro-Magnons" off the main line of human ascent. Not only did Homo sapiens lack direct ancestors, but Gregory also depicted a hominid radiation which had several distinct species of hominid in existence at the end of the Pliocene. Ironically, Gregory made use of the than newly discovered Australopithecus to support his polyphyletism. If, as Elliot Smith was then claiming and Gregory was inclined to accept, Australopithecus was more than a new type of ape and displayed an "advance towards the intellectual supremacy of the human family," then it would have become the fourth distinct type of primitive human or proto-hominid form in late Pliocene and early Pleistocene strata. Such diversity seemed to provide solid support for polyphyletic evolutionary scenarios perhaps, but it should be noted that like most other proponents of this viewpoint Gregory provided no ecological or adaptational explanation for it

Age of Man, p. 46).

153. Gregory, "Two Views of the Origin of Man," Science 65 (1927): 601-605; "How Near is the Relationship of Man to the Chimpanzee-Gorilla Stock," Proceedings of the American Philosophical Society 66 (1927): 439-463; "The Upright Posture of Man: a Review of its Origin and Evolution," Proceedings of the American Philosophical Society 67 (1928): 339-376; "Were the Ancestors of Man Primitive Brachiators?," Proceedings of the American Philosophical Society 67 (1928): 129-150; "Is the Pro-Dawn Man a Myth?," Human Biology 1 (1929): 153-165; "A Critique of Professor Osborn's Theory of Human Origin," American Journal of Physical Anthropology 14 (1930): 133-164; "The Origin of Man from a Brachiating Anthropoid Stock," Science 71 (1930): 645-650.

154. Gregory, "How Near the Relationship," pp. 557-558; "Ancestors Primitive Brachiators," pp. 143-144, 136-139.

155. Gregory, "Origin From the Anthropoid Stem," pp. 450-451; "Origin from Brachiating Anthropoid Stock," p. 647, "Ancestors Primitive Brachiators", p. 135.

156. Morton, "Evolution of the Human Foot; I and II," American Journal of Physical Anthropology 5 (1922): 305-336, and Ibid. 7 (1924): 1-52.

157. Gregory, "Ancestors Primitive Brachiators," p. 136; "How Near the Relationship," p. 559; "Origin from the Anthropoid Stem," p. 461.

158. Gregory, "Origin from Brachiating Anthropoid Stock," p. 650.

159. Gregory, "Origin from the Anthropoid Stem," p. 461.

160. Gregory, "Origins from the Anthropoid Stem," p. 461; on "neokinesis", see Tilney, Brain from Ape to Man, pp. 700-725.

161. Gregory, "Origin from Brachiating Anthropoid Stock," p. 649; it is interesting to note that Schultz was more cautious in drawing phylogenetic conclusions from his growth studies than either Gregory or Osborn. Schultz did accept Gregory's contention that to enumerate the developmental changes undergone by the fetal human foot would be "to repeat, word for word, the hypothetical transformation of the foot of a gorilla into that of a

man." Nevertheless, Schultz argued, when placed in the context of the many resemblances among higher primates generally, these findings merely confirmed the existence of common ontogenetic patterns and did not prove any particular phylogenetic relationships among apes and humans. In addition, Schultz' judgement about the existence in humans of characters that were "less specialized" and "more original and 'primitive'" than "various other" higher primates can be seen as a jumping off point for the later arguments of Schultz' junior colleague, W.L.Straus, Jr., who sought to demonstrate that man's relatively primitive features rendered a "brachiating" ancestry unlikely. See Schultz, "Growth Studies," p. 162, and pp. 583-585 below.

162. Gregory, "Two Views," p. 601.

163. Gregory, "Pro-Dawn Man," p. 156; "Critique of Osborn," p. 140.

164. Gregory, "How near the Relationship," pp. 557-558.

165. Gregory, "Pro-Dawn Man," p. 158; "Ancestors Primitive Brachiators," pp. 139-140.

166. Gregory, "Two Views," p. 602; "Origin From the Anthropoid Stem," p. 455.

167. Gregory, "Origin from the Anthropoid Stem," pp. 443-451, quotation from p. 451.

168. Gregory, "Critique of Osborn," p. 155.

169. Ibid., pp. 139, 137; Gregory, "Two Views," p. 605.

170. Henry Fairfield Osborn, Titanotheres of Ancient Wyoming, Dakota, and Nebraska, U.S. Geological Survey Monograph no. 55 (Washington, D.C., 1929), pp. 41-45. In present day terminology, it should be noted, allometry appears to have a more restricted meaning -- i.e. it refers to those changes in form and proportion that are correlated with the factor of relative body size. See, for example, David Pilbeam and Stephen Jay Gould, "Size and Scaling in Human Evolution," Science 186 (1974): 892-901.

171. Gregory, "Pro-Dawn Man," pp. 154-155; "The Roles of Undeviating Evolution and Transformation in the

Origin of Man," American Naturalist 69 (1935): 385-404.

172. Gregory,"Origin from the Anthropoid Stem," p. 462; "Did Man Originate in Central Asia?," Scientific Monthly 24 (1927): 385-401, p. 399. In fact, in 1934 Gregory described Davidson Black's synthesis of the theory of paedomorphosis with the extant evidence on fossil hominids as a "masterly" one. See Gregory, Man's Place among the Anthropoids (Oxford: the Clarendon Press, 1934), p. 94.

173. This theme was especially well developed in a popular book that Gregory published in 1929, Our Face from Fish to Man (New York: Putnam's, 1929). See for example the passage on pp. 75-76 in that work.

174. Gregory, "Critique of Osborn," p. 138; "Pro-Dawn Man," p. 161.

175. Gregory, "Pro-Dawn Man," p. 155.

176. For more on the arboreal theory see pp. 344-347 above.

177. Frederic Wood Jones, "Some Landmarks in the Phylogeny of the Primates," Human Biology 1 (1929): 214-228, p. 214; the use of Gregory's words occurrred in Wood Jones, Man's Place among the Mammals, pp. 36-37, as well as in The Ancestry of Man, pp. 10-11.

178. Gregory, "Evolution of the Human Dentition. Part 5," p. 109.

179. Gregory, "Origin From the Anthropoid Stem," p. 435.

180. Gregory, "A Critique of Professor Frederic Wood Jones' Paper: 'Some Landmarks in the Phylogeny of the Primates,'" Human Biology 2 (1930): 99-108.

181. The full title of Huxley's work was T.H. Huxley, Evidence as to Man's Place in Nature (London: Williams and Norgate, 1963).

182. Gregory, Man's Place, pp. 1-32; the conclusions are on pp. 31-32.

183. Ibid., pp. 36-45.

184. Ibid., pp. 97-98.

185. Ibid., pp. 98-100.

186. Ibid., pp. 101-103.

187. Ibid., p. 93.

188. Ibid., p. 117.

189. Ibid.

190. Ibid., p. 112.

191. Ibid., p. 104.

192. See pp. 593-594 below.

193. Gregory, Man's Place, pp. 105-106.

194. Ibid., p. 112.

195. Ibid., p. 113.

196. This is not to say, however, that Gregory's view of the emergence of various parts of the human adaptive pattern coincides with recent opinion. In particular, his acceptance of Piltdown pushed him toward a mistaken belief in the early acquisition of large brain size.

197. See note 64 in the chapter on Hrdlicka, and on Hooton pp. 425-427 above; also consult Fleagle and Jungers,"Fifty Years of Higher Primate Phylogeny," pp. 195-201.

198. Gregory, "Evolution of the Human Face. Chief Stages in its Development from the Lowest Forms of Life to Man," American Museum Journal 17 (1917): 377-388; "The Evolution of the Human Face," Natural History 19 (1919): 421-425.

199. Gregory, "The Paleomorphology of the Human Head. Ten Structural Stages from Fish to Man," Quarterly Review of Biology 2 (1927): 267-279.

200. Gregory,"Paleomorphology," p. 270.

201. Gregory, "Basic Patents in Nature," Science 78 (1933): 561-566, p. 565.

202. Gregory, "Undeviating Evolution," p. 387.

203. Ibid., pp. 398-404.

204. Ibid., p. 388.

205. Ibid., pp. 389, 403.

206. Gregory, "Ployisomerism and Anisomerism in Cranial and Dental Evolution among Vertebrates," Proceedings of the National Academy of Sciences 20 (1934): 1-9, p. 9.

207. Gregory, "Polyisomerism and Anisomerism," p. 8, and "Polyisomerism and Anthropogeny," Human Biology 6 (1934): 632-636, p. 632.

208. Gregory, "Polyisomerism and Anisomerism," p. 4; "Polyisomerism and Anthropogeny," p. 636.

209. Gregory, "Fish Skulls: a Study of the Evolution of Natural Mechanisms," Transactions of the American Philosophical Society 23, parts i-vii (1933): 75-481; see Colbert, "William King Gregory," p. 100, for an estimate of its importance.

210. Gregory, "Fish Skulls," pp. 412-449.

211. Ibid., p. 450.

212. Ibid.

213. Ibid., p. 454. Since genetic ideas are not a main concern here, I cannot go into this issue in detail, but it seems that without some concept of the breeding populations and isolating mechanisms involved in these transitions, the efficacy of the mechanisms Gregory was sketching seems hard to evaluate. It is interesting to note, though, that this concern with interacting systems of genes and the possibility they held out for rapid evolution were important facets of the work in the early 1930s of Sewall Wright. Gregory mentioned Wright's work elsewhere in his writings. In "Fish Skulls" he may have been giving his own non-mathematical and somewhat traditional translation of these views. See Bowler, Evolution, pp. 235-236.

214. Gregory, "Fish Skulls," pp. 75-77.

215. Gregory, "The Pelvis from Fish to Man: a Study

in Paleomorphology," American Naturalist 69 (1935): 193-210, p. 210; W.L. Straus, Jr., "Studies on Primate Ilia," American Journal of Anatomy 43 (1929): 403-460.

216. For more sophisticated versions of the idea I am trying to present, see W.E. LeGros Clark, The Antecedents of Man (New York: Harper and Row, 1936), pp. 11-17, and Ernst Mayr, "Cladistic Analysis or Cladistic Classification?, pp. 433-476 in Mayr, Evolution and the Diversity of Life: Selected Essays (Cambridge: Harvard University Press, 1976), pp. 471-473 especially.

217. An obvious illustration of this problem is provided by the australopithecine pelvis. Though similar in many ways to that of Homo sapiens , it is hardly identical to the latter. Yet if recent biomechanical studies are on target, the australopithecines possessed a highly efficient pattern of bipedal adaptation. See Owen Lovejoy, "The Origin of Man," Science 211 (1981): 341-350, pp. 345-346.

218. See for example, the review of recent literature on these problems by Henry W. McHenry, "The Pattern of Human Evolution: Studies on Bipedalism, Mastication, and Encephalization," Annual Review of Anthropology 11 (1983): 151-173.

219. On Hrdlicka, see Boaz, "Early Hominidae," p. 399, and on Hooton, p. 366, and pp. 420-421 above.

220. Gregory, "How Near the Relationship to the Chimpanzee Gorilla Stock," p. 559.

221. Gregory, "Pro-Dawn Man," p. 156.

222. Gregory, "Origin from Brachiating Anthropoid Stock," p. 650.

223. Ibid.

224. Gregory, "Critique of Osborn," p. 139.

225. Gregory with M. Mok, "How Man-Apes Became Men a Million Years Ago," Popular Science Monthly 119, no. 4 (1931): 22-24, 134-136, pp. 24, 134.

226. Gregory, Man's Place, pp. 105-106; "The Origin, Rise and Decline of Homo Sapiens ," Scientific Monthly 39 (1934): 481-496, pp. 494-496. Dart's view came through particularly clearly in an article he addressed to

the American audience in 1926. See Raymond Dart, "Taungs and Its Significance," Natural History 26 (1926): 315-326. His original report was Dart," Australopithecus africanus : the Man-Ape of South Africa," Nature 115 (1925): 195-199.

227. Gregory, "How Man-Apes," p. 22.

228. That Gregory was still, in 1931, sheepish over this fiasco came out clearly in the Popular Science interview, where the "Hesperopithecus" was hyperbolically described as the "worse sic booby trap case on record" (p. 134).

229. For the cautious, but generally favorable hearing that Dart received in America in the years just after the discovery of the Taungs fossil, see Boaz, "Early Hominidae," pp. 399-400.

230. Broom discussed his initial findings in "On Australopithecus and its Affinites," in George Grant MacCurdy, ed. Early Man (Philadelphia: Lippincott, 1937), pp. 285-292, and "The Pleistocene Anthropoid Apes of South Africa," Nature , 142 (1938): 377-379. His later synthesis of the australopithecine material is contained in R. Broom and G.H.W. Schepers, The South African Fossil Ape-Man, the Australopithecine. Transvaal Museum Memoir no. 2 (Pretoria, 1949).

231. Gregory and Milo Hellman, "The South African Man-Apes and the Origin of the Human Dentition," Journal of the American Dental Association 26 (1939): 558-564, pp. 558-559.

232. Gregory and Milo Hellman, "The Dentition of the Extinct South African Man-Ape Australopithecus (Plesianthropus) Transvaalensis Broom. A Comparative and Phylogenetic Study," Annals of the Transvaal Museum 19 (1939): 339-373., p. 364.

233. Gregory and Hellman, "Dentition of the Extinct Man-Ape," p. 364.

234. Franz Weidenreich. "The Dentition of Sinanthropus Pekinensis ; a Comparative Odontography of the Hominids," Paleontologia Sinica, New Series D, No. 1 (Peiping, 1937).

235. Gregory and Hellman, "Dentition of the Extinct Man-Ape," p. 364. Adolf Remane, "Morphologie des

Anthropoidengebisses."

236. Gregory and Hellman, "Dentition of the Extinct Man-Ape," p. 367.

237. Ibid., p. 365.

238. Ibid.; and Gregory and Hellman, "The South African Man-Apes," p. 366.

239. Gregory and Hellman, "Dentition of the Extinct Man-Ape," p. 366.

240. Ibid.; Gregory and Hellman, "The South African Man-Apes," p. 561; "The Upper Dental Arch of <u>Plesianthropus Transvaalensis</u> Broom, and its Relations to Other Parts of the Skull," <u>American Journal of Physical Anthropology</u> 26 (1940): 211-228, pp. 214-217.

241. Gregory and Hellman, "Dentition of the Extinct Man-Ape," p. 366, and "The South African Man-Apes," p. 562.

242. Gregory and Hellman, "Dentition of the Extinct Man-Ape," p. 367; "The South African Man-Apes," p. 562.

243. Grgory and Hellman, "Dentition of the Extinct Man-Ape," p. 367.

244. Ibid., pp. 360-362; Gregory and Hellman, "The Upper Dental Arch," p. 217.

245. Gregory and Hellman, "The South African Man-Apes," p. 562.

246. Gregory and Hellman, "Dentition of the Extinct Man-Ape., p. 365.

247. Gregory and Hellman, "The Upper Dental Arch," p. 215.

248. Gregory and Hellman, "The South African Man-Apes," p. 563.

249. Gregory and Hellman, "The Upper Dental Arch," p. 217.

250. Gregory and Hellman, "Dentition of the Extinct Man-Ape," p. 365.

251. Ibid., p. 365; Lewis' evaluation of Ramapithecus was included in G.E. Lewis, "Taxonomic Syllabus of Siwalik Fossil Hominoids," American Journal of Science 27 (1937): 161-181.

252. Gregory and Hellman, "The Upper Dental Arch," p. 228.

253. Gregory and Hellman,"The South African Man-Apes," p. 563.

254. Gregory and Hellman, "Dentition of the Extinct Man-Ape," p. 364.

255. Ibid., p. 367, 365.

256. Ibid., pp. 367-368.

257. Ibid., p. 368; "The South African Man-Apes," p. 368.

258. Gregory and Hellman, "The South African Man-Apes," p. 564; "Dentition of the Extinct Man-Ape," 368.

259. Gregory and Hellman,"The South African Man-Apes," p. 560.

260. Ibid.

261. Gregory and Hellman, "Dentition of the Extinct Man-Ape," p. 370.

262. Ibid., p. 371. The authors used similar language in a brief article in Science summarizing their findings:

> While the myth of the Eocene dawn man will continue to flourish, the small-brained man-apes of South Africa now add their mute testimony that man, like his less ambitious cousins, the modern anthropoid apes, is a descendant of the late Tertiary dryopithecine ape stock of Europe, Asia and Africa, and that, as long maintained by us and more recently supported by Davidson Black, Weinert and Broom, the human status was gained through a long continued an profound morphological revolution during the Pliocene and early Pleistocene epochs.

Gregory and Hellman, "Evidence of the Australopithecine

Man-Apes on the Origin of Man," Science 88 (1938): 615-616, p. 616.

263. Gregory and Hellman, "Revised Reconstruction of the Skull of Plesianthropus Transvaalensis Broom," American Journal of Physical Anthropology n.s. 3 (1945): 267-275, p. 270.

264. Gregory, "The Bearing of the Australopithecinae upon the Problem of Man's Place in Nature," American Journal of Physical Anthropology, n.s. 7 (1949): 485-512.

265. Ibid., pp. 504-505.

266. Ibid., p. 488.

267. Ibid., pp. 488-490.

268. Ibid., p. 492.

269. Ibid., p. 503.

270. Ibid., p. 501.

271. Ibid.

272. Ibid., p. 508.

273. Ibid., p. 510.

274. Gregory, Evolution Emerging. A Survey of Changing Patterns from Primeval Life to Man (New York: Macmillan, 1951).

275. Ibid., p. ix.

276. Ibid., pp. ix, x.

277. Ibid., p. x.

278. Ibid., pp. 485-490; the quotation on the pelvis is from p. 486, those on Schepers from pp. 533 and 538. Sharp criticism of Schepers' methods and the conclusions resulting was later voiced other scientists, for example by the eminent neuroanatomist Gerhardt von Bonin. See von Bonin, The Evolution of the Human Brain (Chicago: University of Chicago Press, 1963) p. 25.

279. Gregory, Evolution Emerging, p. 533.

280. Ibid., p. 490.

281. See Gregory, "Franz Weidenreich, 1973-1948," American Anthropologist 51 (1949): 85-90.

282. See p. 492 above.

283. Gregory, Evolution Emerging, pp. 492, 493, 495.

284. Franz Weidenreich, Apes, Giants and Man (Chicago: University of Chicago Press, 1946), and "Giant Early Man from Java and Southern China," Anthropological Papers of the American Museum of Natural History 40, pt. 1 (1945). For the circumstances surrounding these important finds, see G.H.R. von Koenigswald, "The Discovery of Early Man in Java and Southern China," in W.W. Howells ed., Early Man in the Far East (Philadelphia, 1949), pp. 83-101.

285. Weidenreich, Apes, Giants and Man, pp. 46-66.

286. See pp. 491-492 above.

287. Gregory, Evolution Emerging, p. 491.

288. Ibid., p. 494.

289. Ibid., p. 495.

290. Weidenreich, The Skull of Sinanthropus, pp. 216-220.

291. Gregory, Evolution Emerging, p. 498.

292. Ibid., p. 498.

293. Ibid.; see for example p. 448 above.

294. Gregory, Evolution Emerging, p. 498.

295. Ibid., p. 498.

296. Ibid., pp. 478-480, quotation from p. 478; for a recent evaluation of Proconsul from an expert on Miocene hominoids, see David Pilbeam, "Major Trends in Human Evolution," in Lars Konig Konigson ed., Current Argument on Early Man (Oxford: Pergamon Press, 1980) p. 261-285, especially pp. 270-272.

297. Gregory, Evolution Emerging , p. 482.

298. Ibid., pp. 482, 484.

299. Ibid., p. 484.

300. Ibid., pp. 534-536.

301. Ibid., pp. 553-554, 548-553, 558-559.

302. Ibid., p. 15.

303. Ibid., p. 536.

304. Ibid., pp. 557-558.

305. Sherwood L. Washburn, "The New Physical Anthropology," Transactions of the New York Academy of Sciences, ser. II, 13 no. 7 (1951): 298-304, and "The Strategy of Physical Anthropology," in A.L. Kroeber, ed. Anthropology Today (Chicago: University of Chicago Press, 1953).

306. On the distinction between these two kind of scientific endeavor, see Karl Popper, The Poverty of Historicism , 3rd. ed. (New York: Harper and Row, 1961), The Logic of Scientific Discovery (London: Hutchinson, 1959).

307. The quotation is from Eliot's essay, "Tradition and the Individual Talent," cited in Denis Donoghue, "Newton's Other Law: Glory Is the Real Reward," New York Times Book Review , April 21, 1985, p. 34.

Conclusion

1. This is the major point stressed in Spencer and Smith,"The Significance of Aleš Hrdlička's 'Neanderthal Phase of Man,'" and Spencer, "Aleš Hrdlička, M.D."

2. Weidenreich's contribution lay not only in his masterful description and analysis of the Choukoutien "Sinanthropus" population, but also in influential theoretical articles such as Weidenreich, "The

'Neanderthal Man' and the Ancestors of 'Homo Sapiens,'" American Anthropologist 45 (1943): 39-48; "Generic, Specific and Subspecific Characters in Human Evolution," American Journal of Physical Anthopology, n.s. 4 (1946): 413-422; and "Facts and Speculations Concerning the Origin of Homo Sapiens, " American Anthropologist 49 (1947): 187-203. Dobzhansky's seminal article was "On Species and Races of Living and Fossil Man," which was preceded by the influential theoretical work, Genetics and the Origin of Species (New York: Columbia University Press, 1937). Mayr also produced a highly influential article on hominid evolution, "Taxonomic Categories in Fossil Hominids," preceded by a general theoretical work that had great impact on paleoanthropology, Systematics and the Origin of Species (New York: Columbia university Press, 1942). Mention should also be made of the pioneering "neo-Darwinian" work of the paleontologist George Gaylord Simpson, Tempo and Mode in Evolution (New York: Columbia University Press, 1944). Trinkaus," Homo erectus and Homo sapiens Paleontology in America," pp. 266-267 gives a brief appreciation of the influence of the latter three figures on post-World War II paleoanthropology.

3. See, for example, Boaz, "American Research on Australopithecus, " pp. 249-250, which notes the great importance that these new approaches to the study of fossil hominids have attained.

4. Ruse, Darwinism Defended, pp. 224-226.

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GLOSSARY

Acheulian. The best known Lower Paleolithic industry, which is generally characterized by the presence of large numbers of more or less symmetrical hand-axes. After appearing in East Africa in the Lower Pleistocene, the Acheulean lasted well into the Middle Pleistocene in Africa, Europe and the Near East.

Anthropoid. Technically a member of the Primate suborder that includes the Old World monkeys, apes and hominids, both fossil and recent. In common parlance it has often been used as an adjective synonymous with "ape-like."

Artifact. Any object that has been made, modified or used by human beings.

Assemblage. A group of objects found in association with each other in a single geological deposit, and which are therefore assumed to have belonged to a single human group.

Association area. A region of the cerebral cortex believed to be involved in the connection of impulses emerging from other cortical regions that are more directly concerned in specific sensory and motor functions.

Aurignacian. An earlier Upper Paleolithic industry from Europe, and France in particular, which in the early 20th century was seen as the the first "stage" in cultural evolution associated with anatomically modern *Homo sapiens*.

Australopithecus africanus. The name given by Raymond Dart in 1925 to the first australopithecine fossil discovered, the so-called "Taungs baby." The name is now applied to gracile australopithecine skeletons found in both South and East Africa.

Biogeography. The study of the geographical distribution of particular kinds of plants and animals, and of the floras and faunas of which they are a part.

Blade Tools. Tools produced from long and narrow stone flakes. Techniques for preparing flint cores so that numerous, high quality blades could be produced from a single piece of stone are characteristic of Upper Paleolithic industries.

Brachiation. The art of locomotion through the trees by means of the forelimbs.

Brachycephalic. Having a head that is relatively short and broad, particularly as measured by the cephalic index.

Canines. The teeth lying just posterior to the incisors in the primate dentition, often employed either for grasping food or, when enlarged and tusklike, for fighting.

Cephalic index. A measure of the overall shape of the head, obtained by dividing the maximum length of the skull by the maximum width, and multipyling the resulting ratio by 100.

Chellean. The term commonly used in the early 20th century for the Abbevillean, a lower Paleolithic industry that is an earlier and less developed form of the Acheulian.

Core. The part of a nodule of flint, or other stone suitable for use as a tool, that remains after one or more flakes has been struck off. Hand axes are the most familiar type of paleolithic tool produced from cores.

Cusp. A protuberance on the surface of a tooth that articulates with the teeth in the opposite jaw.

Cro-Magnon. The term originally referred to a sample of anatomically modern fossil skeletons found in a cave shelter in the French hamlet of that name in 1868. These skeletons later became widely accepted as the type specimens of the "race" of humans present in Europe in the Upper Paleolithic.

Dentition. The teeth.

Dolicocephalic. Possessing a head that is relatively long and narrow, particularly as measured by the cephalic index.

Dryopithecus pattern. A distinctive arrangement of the cusps and furrows on the lower molars of hominoids discovered by William K. Gregory. It was so named because Gregory argued that the pattern appeared first in specimens of the fossil hominoid genus Dryopithecus.

Eocene. The second, and longest period of the Tertiary era, believed to have lasted from 58 million to 35 million years ago.

Eolith. A term derived from Greek words meaning "dawn stones," applied in the late 19th and early 20th centuries to stones that might have been used, though not intentionally shaped or modified by early hominids.

Encephalization. The process by which brain size in a group of animals increases over evolutionary time at a relatively faster rate than body size, resulting in higher intelligence.

Endocranial cast. A cast of the interior of the cranial cavity, often used by paleoanthropologists to estimate the size and form of the brain of fossil hominids.

Glacial. Of or pertaining to ages in the earth's history when glaciers have made major advances over the terrestrial surface. Glaciation is the process of glacial advance.

Günz. The first of the four major epochs of glacial advance in the Alpine region of Europe. It is usually placed in the lower Pleistocene and dated between 1,000,000 and 600,000 years before the present.

Fauna. The sum of the animal species living in a given location at a specific point in time.

Flake. A relatively thin piece of flint or other stone that has been struck from a core or larger flake by a single blow.

Frontal lobe. The anterior portion of each cerebral hemisphere, believed in the earlier part of the 20th century to be responsible for higher mental processes such as planning and foresight.

Hand-axe. An unspecialized stone tool made by removing flakes from both faces of a core in order to produce an oval or pointed form with a continuous, symmetrical cutting edge.

Hominid. A creature belonging to the family within the primate order which includes humans and their immediate fossil relatives, i.e. those appearing after the last common ancestor of present day human and non-human forms.

Hominoid. A creature belonging to the superfamily within the primate order which includes the great apes and hominids.

Homo heidelbergensis. The name customarily given to the well-preserved hominid jaw found at Mauer, near Heidelberg, Germany in 1905. The fossil is now commonly attributed to Homo erectus, but the possibility that it represents an archaic form of Homo sapiens has also been raised.

Homo neanderthalensis. The preferred generic and specific designation during the early 20th century for the Neanderthals of western Europe, when they were widely thought to represent a species separate from Homo sapiens. The name was first applied to the Neanderthal type specimen from the Neander valley near Dusseldorf, Germany by William King in 1864.

Homo rhodesiensis. The name originally given to a partial skeleton of a fossil hominid discovered at Broken Hill, Zambia in 1921. The fossil has generally been classed in recent years as belonging to an archaic form of Homo sapiens slightly more primitive than Neanderthal man.

Incisors. The most anterior teeth in the primate dentition, whose function is usually the cutting of food.

Industry. A set of artifacts of a single class (such as pottery or flint tools) which are thought to have been produced by a particular human group or society.

Interglacial. One of the geological epochs lying between the main glacial epochs of the Pleistocene. The climate during interglacial epochs is assumed to be as warm as or warmer than at present.

La Chapelle aux Saints. A fairly complete and well-preserved "classic Neanderthal" skeleton found at a site of the same name in southwestern France in 1908. Marcellin Boule's studies of this skeleton did more than anything else to define the Neanderthal "type" in the early 20th century.

Levalloisian. A technique for striking large flake tools from a specially prepared flint core, one that has taken on a distinctive "tortoise shell" shape. The Levalloisian appeared in Europe contemporaneously with the later stages of the Acheulian industry and prior to the appearance of the Mousterian.

Lower Paleolithic. The earlier division of the Paleolithic that encompasses the stone tool industries of the early and middle Pleistocene from the Oldowan of East Africa through the Acheulian.

Magdalenian. A later Upper Paleolithic industry of Europe, which was taken to represent the stage in the evolution of culture following the Solutrean, and was characterized by highly developed techniques of blade tool production and especially by complex implements of bone and antler.

Mandible. The bones that form the lower jaw and carry the lower dentition.

Maxilla. The bone of the face that makes up the upper jaw and carries the upper dentition.

Middle Paleolithic. The period of Stone Age culture commonly associated with Homo sapiens neanderthalensis and usually dated from about 70,000 to 35,000 years before the present. The period is especially marked by wide usage of a variety of flake tools made from prepared cores and by the first appearance of a significant degree of regional cultural specialization.

Mindel. The second of the major glacial epochs in the Alpine regions of Europe during the Pleistocene. It is usually placed in the Middle Pleistocene and dated from around 500,000 to 350,000 years before the present.

Miocene. The geological period of the Tertiary preceding the Pliocene, at present believed to have lasted from about 25 milliion to 5 million years ago.

Molars. The posterior teeth in the primate dentition, which are adapted to grinding food.

Mount Carmel. A location in Palestine where cave excavations in the late 1920s and early 1930s uncovered several important fossil hominid skeletons, some of which seemed either to bridge the morphological gap between the Neanderthals and anatomically modern humans or to present a mixture of characters common to each group.

Mousterian. The Middle Paleolithic industry commonly associated with finds of "Neanderthaloid" fossils in Europe. The most distinctive characteristic of the Mousterian in relation to preceding industries is the presence of numerous types of specialized flake tools such as points, scrapers and knives struck from prepared cores.

Occipital Lobe. The part of each cerebral hemisphere which is located at the back of the head and is principally concerned with vision.

Oligocene. The third geological period of the Tertiary era, believed to have lasted from about 35 million to 25 million years ago.

Paleolithic. The period of human culture also known as the Old Stone Age. It has been traditionally understood as beginning with the introduction of the first recognizable stone tools and ending just prior to the domestication of plants and animals.

Paranthropus. The generic name given by Robert Broom to his highly important finds of robust australopithecine fosils at Swartkrans, South Africa. These fossils are now generally assigned to the species *Australopithecus robustus*.

Parietal lobe. The part of each cerebral hemisphere lying between the frontal and occipital lobes.

Phylogeny. The evolutionary history and lineage of an organism or group of organisms.

Pithecanthropus erectus. The name given by Eugene Dubois to the fossil hominid skull he discovered at Trinil in Java in 1891. The specimen is now classified as belonging to *Homo erectus*, the hominid species ancestral to *Homo sapiens*.

<u>Pleistocene.</u> A geological period of the Quaternary era, which is now estimated to have begun about 1.8 million years ago and ended about 10,000 years ago. It is especially connected with the most recent major glacial epochs in the earth's history.

<u>Plesianthropus transvaalensis.</u> The name given in 1937 by Robert Broom to the finds of gracile australopithecine fossils made at Sterkfontein, South Africa. These fossils are now considered to be representatives of <u>Australopithecus africanus.</u>

<u>Pliocene.</u> The last geological period of the Tertiary era, at present believed to have lasted from about 5 million to 1.8 million years ago.

<u>Pongid.</u> A term derived from the Latin name for the orangutan <u>(Pongo)</u> that has commonly been applied to the large-bodied apes gorilla, chimpanzee and orangutan in general.

<u>Premolars.</u> The teeth lying between the canines and molars in the primate dentition. In apes and Old World monkeys the anterior lower premolars are adapted to cutting, while humans have tended to lose this specialization.

<u>Quaternary.</u> The most recent geological era, which includes the Pleistocene period as well as the Holocene, or recent time.

<u>Riss.</u> The third of the major Pleistocene glacial epochs in the Alpine regions of Europe. It is usually placed in the Middle Pleistocene and dated from around 250,000 to 125,000 years before the present.

<u>Skhūl.</u> A cave in the Mount Carmel region of Palestine which yielded fossil remains of individuals which seemed to possess a mixture of "Neanderthaloid," anatomically modern and intermediate characters.

<u>Solutrean.</u> An Upper Paleolithic industry of Europe, which in the early 20th century was seen to represent the stage in the evolution of culture following the Aurignacian. The Solutrean is especially characterized by finely worked and highly symmetrical leaf shaped projectile points.

<u>Steinheim.</u> An important Middle to Late Pleistocene hominid skull discovered in Germany in 1933. It is

generally seen as a representative of "archaic" Homo sapiens ancestral to Neanderthal man.

Strata. Layers of rock in a geological deposit.

Stratigraphy. The study and mapping of geological deposits with a view toward the establishment of the relative ages of the various layers and of their relationships to other deposits.

Supraoribital torus. A ridge of bone extending above the orbits, or eye sockets which is well-developed in African great apes and primitive hominids but less so in anatomically modern humans.

Swanscombe. An incomplete fossil hominid skull found in 1935 at a site in Kent, England of the same name. At first the skull was commonly classed as a representative of modern Homo sapiens but eventually its close similarities with more complete specimens believed to be "archaic" forms of Homo sapiens ancestral to the Neanderthals cast this interpretation into doubt.

Synthetic theory. The neo-Darwinist theory of evolution by means of genetic variation and natural selection that gained prominence in various disciplines in biology in the years after 1940.

Tabūn. A cave in the Mount Carmel region of Palestine, where hominid fossils were discovered around 1930, including a fairly complete female skeleton similar in overall appearance to the "classic" Neanderthals of western Europe.

Temporal Lobe. The division of each cerebral hemisphere lying low down on the side of the brain, believed in the earlier part of the 20th century to be specially concerned with memory and speech comprehension.

Tertiary. The geological era preceding our own, the Quaternary. The Tertiary is characterized by the radiation of the mammals, and is believed to have lasted from about 63 millon to 1.8 million years ago.

Upper Paleolithic. A group of industries which followed the Mousterian and similar Middle Paleolithic industries in the late stages of the Pleistocene. Upper Paleolithic industries have typically been characterized by the presence of sophisticated "blade" tools in stone and various bone and antler tools of a high degree of workmanship.

<u>Würm.</u> The last epoch of major glacial advance in Alpine regions of Europe. It is generally estimated to have lasted from around 50,000 to 10,000 years before the present.